

How to Find a European Space Policy

THE future of space research and development in Europe remains unclear, although there is just an outside chance that the meeting of responsible ministers in Paris this week (see page 64) may have helped to remove some of the impediments to an efficient expenditure of resources in this field. The essential difficulty is that European governments have nothing like an agreement on what they want to accomplish. The British government, for example, has set its face against the development of launching vehicles, but is in principle in favour of the development of satellites, not merely for purposes of scientific research but for applications in communications and meteorology. By contrast, the French government feels keenly that an independent launching capability in Europe is politically if not technically necessary and has as a result been urging that ELDO should push ahead with making a success of the Europa rocket and has, for good measure, persisted independently with its own development of rockets. But Germany and Italy as well as other European nations have chosen to sit on the fence, and appear to share the British scepticism of the value of an independent rocket development while profiting, technically if not financially, from the work that ELDO continues to sponsor. The prospect that Europe might have a space policy thrust upon it in the shape of the post-Apollo programme has, in the past few months, mercifully receded—opportunities for collaboration with NASA have dwindled as the months have dragged by since European collaboration was first invited, with the result that a programme of satellite development is now likely to yield benefits at least as good as those of participation in the shuttle service. Procrastination evidently has some virtue.

But European governments seem dangerously unable to tackle even straightforward administrative problems such as the need to marry ESRO (European Space Research Organization) and ELDO (European Launcher Development Organization) which have been apparent since 1968. And in the past few weeks, a new challenge to their sagacity has been provided by the announcement early in October from the White House that, with certain reservations, the United States will in future be prepared to provide launching facilities for satellites manufactured independently elsewhere (see *Nature*, **239**, 424; 1972). The first thing to be said is that the terms of the American announcement are not nearly as generous as they seem and that they are, by themselves, insufficient to turn the arguments which the government of France has been making, in recent years, for the independent development of rockets in Europe. President Nixon said on October 9 that he would provide launching facilities for other governments and for international organizations so long as the objectives were peaceful, but that telecommunications satellites would be included only if they were first recommended by Intelsat or supported by the United States within Intelsat. What this implies is that there is no way of predicting in advance whether, for example,

it would be possible for European nations to consider collaborating on a regional television satellite which might be launched with an American rocket. Everything would depend on whether Intelsat would approve, and although the Canadian communications satellite launched two weeks ago may seem a hopeful precedent, and although European nations are strongly represented on the board of Intelsat, there is always at least a risk that such a project would be turned down. Certainly European governments which hold, as does the French, that a facility for independence is an emotional as well as a political necessity, will not easily trust themselves to the provisions of the American offer, especially while Comsat remains the manager of Intelsat.

The logic of the British case against the independent development of launching vehicles is, however, unshakable. It is an expensive business and few would argue that long-term European interests would be served by repeating American technical developments of the past decade. But the question is not nearly as stark a choice of alternatives as the European governments have been tempted to suppose. Without apparent difficulty, for example, the government of Japan has been able to negotiate with the United States an agreement for using the technology of the Thor rocket as a means of launching communications satellites later in the decade. Is there any reason why the governments of Europe should not be able to negotiate a similar or even a better agreement? And even though licence fees would no doubt in the end be payable, the cost would certainly be less than that of an independent development of a European rocket, while there would be an even more rapid development of European skills in rocket technology as a result. But there is also scope for making Intelsat a more independent and representative body. The agreement that Comsat should be the manager of Intelsat has five more years to run, but the experience of the last bout of negotiations seems clearly to suggest that it is not too soon for European governments to put pressure on their own telecommunications authorities to construct a more equitable balance between the owners of the international satellite system and the management. To be sure, it would be necessary to find some way of making the telecommunications authority act more closely in concert but that, in any case, is necessary in its own right.

The problem of marrying ESRO and ELDO should also be much simpler than the governments have made it seem. ESRO began as a device for providing facilities for scientific research but has increasingly turned its attention to applications satellites. ELDO began as a device for using the British Blue Streak rocket as a foundation for a European launcher with the unique property of being just powerful enough to put into synchronous orbit payloads too small to be of much practical benefit. It is now under pressure to spread its wings still further, as much from lack of an objective it



can call its own as because of need. But it would be far preferable if the two quite complicated organizations which have grown up in the past decade were not simply married but merged, and if member countries were compelled to agree that membership would not allow them to contract out of parts of the programme with which they did not happen to agree. It must be confessed, of course, that such a happy turn of events would be possible only if there were a thorough and independent study of the basis on which a programme of work might be hammered out, for this is a field in which civil servants, however wise, are unlikely to carry conviction and persuade the sceptics. Until such a study can be completed and approved politically, the European governments should give up the pretence that they can conjure a space programme out of thin air simply by setting a date for the conference postponed from October.

Modest Shuffle

MR EDWARD HEATH has now made the rearrangement of government posts for the new session of parliament that would no doubt have been made a week ago if the government had not been locked in its fruitless discussions with the trades unions and the Confederation of British Industry. Modest though the changes are, their consequences for the management of British science and technology could be considerable, chiefly because of the replacement of Mr John Davies by Mr Peter Walker as Secretary of State at the Department of Trade and Industry. Even before this organization was created by the merging together of the Ministry of Technology and the old Board of Trade, it had become clear that its scale and complexity were a formidable problem of management and of policy making outdone only by the Department of Health, Education and Welfare in the United States, popularly (and rightly) known as the anthill. Mr Davies has not been a great success, even if it may not be entirely his own fault that he was forced to swallow his brave words about the government's unwillingness to help lame ducks, but his experience at the Department of Trade and Industry should help him considerably in his new task of coordinating British government policy towards Europe, for it is clear that the most urgent tasks are grounded in industry and technology. Whether Mr Walker makes a better showing at the Department of Trade and Industry will no doubt depend on his success in preventing too much of his undoubted enthusiasm and energy from being sapped by his passion for trendiness.

There are several urgent issues crying out for his attention. Although the British government is still boasting of its success at the Paris summit meeting in winning approval for a European Regional Development Fund, it remains in something of a muddle about objectives. Is regional policy a device for ensuring that the present distribution of industry in Britain should be preserved indefinitely, or is it a means of helping regions dependent on declining industries over their short-term problems (in which case the link with arrangements for social security should be stronger than it is)? This is an issue into which Mr Walker could quickly sink his teeth. Another is the question of how best to strike a balance

between the department's activities in research and development. In the current financial year, the department is spending £167.9 million on research and development for civil aircraft projects but only £68.7 million on industrial research and development in all other civil fields. Is this a sensible allocation of priorities? Has not the time come for a much harder-headed agreement on the pooling of civil aviation on a European basis? Is there not also scope for similar arrangements where such programmes as the development of fast reactors and the management of standards and metrology laboratories are concerned? And, although the benefits of a more sensible pooling of European effort might not be so much to economize in running costs as to improve efficiency, is it not time that the Department of Trade and Industry began to think of playing a much more decisive part in general research and development in industry as a whole? In the current year, only £14.1 million is set aside for work like this (but the department has found it necessary to lend as much again to International Computers Limited for a similar purpose). It could easily be that Mr Walker's most valuable contribution to the years ahead would be to devise a sensible way of channelling public funds to industry. And then there are specific issues to be decided such as the working out of a clear policy on European space research and development.

The other change in Mr Heath's shuffle which will influence the administration of science and technology is the replacement of Mr William van Straubenzee by Mr Norman St John Stevas at the Department of Education and Science. But here it must be acknowledged that the change is unimportant. In Mr Heath's government, the Department of Education and Science has been unnecessarily monolithic, with Mrs Margaret Thatcher involved in every aspect of the work and with the Parliamentary Under-Secretary of State left with unpalatable tasks such as negotiations with the National Union of Students. But the technical problems of managing higher education and those parts of scientific research left with the department are fearsome and likely to become even more so in the months ahead, as the new Advisory Board on the Research Councils will quickly demonstrate. It would have been sensible of the Prime Minister to have gone back to the pattern of the 1960s and to have appointed a Minister of State with special responsibility for science and higher education.

Power to Which People?

IN the past few months, Mr Tony Benn (alias Mr Anthony Wedgwood Benn) has made most of the running in Britain in public discussion of the nature of democracy and has incurred more derision than such an important subject deserves, possibly because of the manner in which he has argued his case and because of the old-fashioned concepts which run through his arguments. The essence of his case was well put in a speech to the Guild of Newspaper Editors at Bournemouth in April this year, the full text of which has only now been made public. He starts from the premise that the problem of modern democracy is not that of involving people in the processes of government but that of somehow gaining

control of the sources of power in modern government. "The key to this problem . . . is the extent to which we can gain control of the enormous new sources of power which the scientists and engineers have put at our disposal." Mr Benn goes on to argue that the use and abuse of power that derives from the application of scientific knowledge is exactly analogous to the power exercised by the mediaeval barons, that modern industry and the pace of growth and innovation have evoked the need for closer control of industrial enterprises and that there is now an urgent need to strengthen political control of the military and to establish, for the first time, political control over what Mr Benn considers to be the uncontrolled sources of power represented by education and the professions.

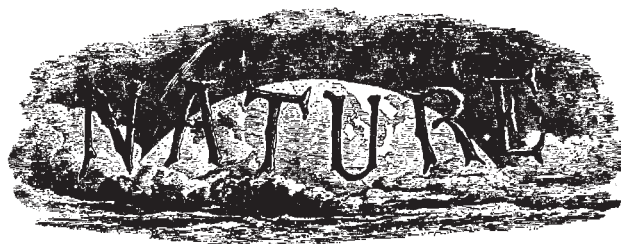
Mr Benn's attack on the education system, and especially the universities, is characteristically overdrawn. He complains that "our educational system is dominated from the top by the universities" and that the universities "in the name of academic freedom have insulated themselves from democratic accountability" behind the shield of the University Grants Committee. He also alleges that a variety of professions—medicine, science, economics and sociology—have sought to insulate themselves against democratic control behind a barricade of academic qualifications and the pretension of expertise. The trouble with Mr Benn's description of the problem of democracy is that his statement of it corresponds only loosely with what the real world is like. In the first place, for example, the University Grants Committee is nothing like the autonomous barony that he describes. For one thing, the committee is often held by parliamentarians themselves to be an enlightened piece of public administration. Parliament has consistently taken the view that Britain needs an independent university system, able to decide for itself where resources should be allocated within a budget fixed by the government. If Mr Benn had complained that the committee's success in creating a more rational pattern within the universities has often fallen short of its own ambitions and of the national need, he would have won and not lost some of his friends. But does he really think that it would be possible, let alone desirable, to take university teachers on to the civil service payroll and to have decisions about priorities in university development complicated by the advocacy by members of parliament of the interests of their local universities? In Mr Benn's complaints against the vested interests of science and the other professions, it is much harder to tell precisely what targets he has in mind. No doubt the research councils would be prominent among his list of baronial vested interests but, as luck will have it, the past year has seen how easily the government, when it chooses, can change the methods by which the research councils are democratically controlled. If anybody is to blame for the slowness with which the need to do this is recognized, it is the government and not the profession of science that should be blamed. Institutions such as the Royal Society might legitimately be complained against on the grounds that they are not representative of or responsive to science as a whole, but here again they are held by government to be a useful means of administering the public interest in science when too formal a link with government as such is a disadvantage—the new International Institute of Applied Systems Analysis is a good example. In short, Mr

Benn's plea that democracy should be made more real is entirely to be supported but his analysis of present defects leaves so much to be desired that those who know his case in detail must doubt the usefulness of his remedies.

Policy for Australia

THE Australian elections on December 2 are likely to be the first for several years in which questions of science policy will have played an important part. Over the years, it has become customary for Australian governments to avoid the question of how best to spend money on scientific research by leaving the job to the Commonwealth Scientific and Industrial Research Organization, the chief channel for public expenditure on research and development in Australia. And nobody will quarrel with the boast of successive governments of Australia that the CSIRO is one of the best organizations of its kind anywhere in the world. In the past few years, however, the organization has increasingly run into criticism, and in recent months it has been fiercely criticized by industrialists for being too detached from practical and commercial considerations. A part of the complaint is that the CSIRO is too often tempted towards unpractical projects, but it is also fair to say, as industrialists have been saying, that applied research carried out within government establishments is bound to be, in some degree, less effective than it would be if it were more fully integrated with production. The Liberal government has this year set up the Advisory Committee on Science and Technology, but the Labour party which now appears to be hopeful of winning power again, would apparently go further. Dr Moss Cass said in a radio interview on October 8 that his party would set up a Ministry for Science and Technology. Experience elsewhere has shown that ministries of science are often effective ways of isolating research and development from the rest of public administration. But even if the Labour Party does now return to power, there is no reason why it should not gracefully forget this election pledge as many other newly elected governments have done before.

100 Years Ago



IN reference to the Swiney Lectureship, which we announced recently as having become vacant, we venture to hope the post will not be thrown away on some one who is already well off, and has taken his place in life, but that it will be given to some young man who has shown himself well qualified for scientific research, and who may thus be enabled to devote his time to investigations which may lead to results of enduring value. Several eminent men have already held this lectureship, including, we believe, Dr. Carpenter.

From *Nature*, 7, 34, November 14, 1872.

OLD WORLD

Post-Apollo Participation Still in Doubt

THE next episode in the sorry saga of Europe's involvement in space is being played out in Paris this week. An informal two-day meeting of ministers responsible for aerospace is being held, at which such perennial hardies as Europe's involvement in the post-Apollo programme and the future of ELDO, the European Launcher Development Organization, are doubtless being mulled over once again.

How far this "informal" meeting is likely to produce any hard decisions is unclear. On past performances little is to be expected. But the meeting may clear the air sufficiently to allow the European Space Conference (whose past two meetings have been postponed at the last minute) to convene some time this year and finally reply to the Americans' invitation to join the post-Apollo programme. The Americans have been very patient. The original deadline for cooperation was July this year, but when July came and went with Europe still vacillating, the deadline was obligingly moved to the end of last month, although the details of what the Americans are offering as possible cooperative projects appear to have changed.

The end of October having passed, with the European Space Conference achieving nothing other than the cancellation of its meeting scheduled for October 26, it appears that the offer is still open, although the Americans seem noticeably less keen to extract a decision from Europe now that the shuttle bill has safely passed the guns of Congress. (International involvement, so the argument ran, would make Congress less likely to cut the programme than if it was simply an all-American venture.)

But if Europe decides to go in now, the benefits could be many times smaller than if a decision had been made soon after the offer was put up two years ago. Originally the extent of European involvement was undefined and the Europeans could have had a sizable say in writing their own ticket, but as time has gone by NASA has tied down the areas in which Europe can play a part more and more tightly; and each time the decision is postponed the area of possible involvement becomes smaller. Now that the October deadline has passed, Europe's proposals for involvement, if they ever come, will have to fit NASA's plans.

But what are the chances of Europe deciding to join America in orbit? Better, it appears, than they were, but still far from good. The Germans—who with France are the mainstay of ELDO—seem to be moving round to the British view that it is better to buy American launchers that work than build European ones that do not. (ELDO's eleven launches in ten years have so far failed to put anything into orbit.) Following a visit to Washington in September by Herr Klaus von Dohnanyi, the West German Minister for Science, the Germans seem much keener than they were on taking an active part in the post-Apollo programme, and Herr von Dohnanyi stated

on German television last Sunday that the Europa programme was a false investment which should be abandoned in favour of a slice of post-Apollo.

But France remains adamant that American assurances that launch facilities will be available are not firm enough, and insists that Europe should develop her own independent launch facilities. It has even been suggested that if the worst came to the worst and ELDO dropped Europa III, then France would consider going it alone. And the situation is not made any easier by the imminence of a general election in Germany which could stand Bonn's space policy on its head again.

So the prospects of European agree-

FRENCH SCIENCE

Closing the Purse Strings

from *La Recherche*

FINANCE for the French five year plan for scientific research has been heavily curtailed. The 1973 budget of 3,710 million francs, just announced, represents an increase of a mere 4.6 per cent over the 1972 budget in a year when inflation has run at about 6 per cent. Under the five year plan, the budget should in fact have risen by 7.5 per cent at constant prices.

The new figures have come as a disappointment to many. Following the tight years of 1970 and 1971, the start of the sixth five year plan last year (see *Nature*, 233, 514; 1971) at least raised hopes for a brighter future.

But while the 1973 budget reflects no overall growth, the situation is less serious in some sectors of research than others. Basic research has been relatively well treated. The Centre National de la Recherche Scientifique (CNRS) will receive Fr.296 million for equipment in 1973—its 1972 budget was only Fr.252 million—although the grants available for university research remain virtually unaltered in 1973. Medical research also does well, the equipment budget for the Institut National de la Santé et de la Recherche Médicale rising from Fr.35 million to Fr.45.7 million. Thus it appears that the policy makers, and in particular the civil servants in the Délégation Générale à la Recherche Scientifique et Technique (DGRST), have managed to protect the long-term interests of basic research, even though the budget is a tight one.

But the *grands programmes* of high technology are falling further and further behind. The budget of the Commissariat à l'Energie Atomique (CEA) will hardly rise at all, nor will that of the Centre National d'Etudes Spatiales (SNES), and both still have their problems.

CEA wants to reorientate much of its work to non-nuclear activities, although plans to do this are still embryonic, while CNES is suffering from the problems of the past and the uncertainty of the future with European space policy still in its perennial shambles. France has yet to convince her European partners that the Europa III launcher should be built, but the implications for CNES could be considerable, especially if France decides to embark on the project alone as some senior civil servants wish.

But if life is hard in the fields of high technology, it is even tougher in the industrial world. The *l'aide au développement* programme which was started several years ago to buttress industrial research comes to a halt in the 1973 budget—even though industrial research is one of the areas recommended for emphasis in the sixth five year plan. Finance for the programme is to be cut in 1973 from Fr.210 million last year to Fr.154 million. But with a fine sense of waywardness, the government has decided to put Fr.85 million into the electronic industry. In deciding to support this sector, which time and again has received state subsidies, the government is turning its back on one of the more important objectives of the sixth five year plan—that of stimulating industrial research in such areas as chemistry, mechanics and metallurgy.

ment are still not bright. European involvement in post-Apollo would probably cost about £250 million initially if that involvement is to mean anything, and Europe is therefore unlikely to decide that it can afford both ELDO and the shuttle (not that Britain, who is no longer a member of ELDO, is going to put anything into Europa III). Unless sufficient common ground can be found by the ministers at Paris, a full blown meeting of the European Space Conference would still seem to be just another waste of time.

ENGINEERING

New Concept Needed

A CALL for universities to run more postexperience courses in engineering was made by Sir Brian Flowers, chairman of the Science Research Council, when he delivered the 1972 Maitland Lecture to the Institution of Structural Engineers yesterday.

Sir Brian also extolled the virtues of the SRC's Engineering Board set up in 1969 on the demise of the University Science and Technology Board. One of its jobs, according to Sir Brian, is to ensure that the "natural and desirable development of engineering activity would [not] be hindered . . . by the imposition of criteria more appropriate to research in the pure sciences".

In engineering, continued Sir Brian, there is a need to take into account the needs of industry when deciding what

research the SRC should support in universities and in deciding what kinds of postgraduate training in engineering the SRC should encourage.

Sir Brian also called for greater co-operation between university engineering departments and industry. The universities should be encouraged to set up postexperience courses in topics which the engineering industry needs—or "can be brought to see that it needs", while engineering employers will have to learn not to dismiss instantly techniques and ideas which emanate from university engineering departments.

The engineering board of the SRC, said Sir Brian, is keen to encourage more first degree students in engineering to carry out postgraduate research after first having spent a year or two in industry. At present, 36 per cent of the 1,915 SRC postgraduate students in engineering have spent some time in industry either before taking their first degree or between their first degree and starting their postgraduate course. Such an arrangement, said Sir Brian, "helps to bring about a greater degree of collaboration and understanding between university and industry". But for more students to pass between university and industry a positive change in attitude must take place to ensure that such a transition does not finish an engineer's prospects for a career in industry. This fear, which he had heard expressed, said Sir Brian, could be partly the fault of the universities "but I doubt if industry is blameless".

A survey has shown that only 13 per cent of engineers employed in industry are involved in research and development activities while 17 per cent are engaged in design, 29 per cent on general administration with the remainder divided between production, construction, consulting, commerce and other disciplines. Sir Brian pointed out that with an ever increasing proportion of engineers receiving postgraduate training the traditional PhD degree based only on research is not really appropriate.

To cater for the needs of industry, said Sir Brian, the Engineering Board was considering supporting a PhD with a new concept in engineering—that of "Total Technology". In such a PhD the student would study design, planning, and operational management, as well as carry out some research. Industry, continued Sir Brian, would have to be convinced of the usefulness of such a PhD although most industries would at present be prepared to accept a one-year course in Total Technology as appropriate training for a career in engineering. Companies which had been approached were, however, clear that a person entering upon a Total Technology course would need

some years of industrial experience first.

Sir Brian pointed out that companies are not going to send their employees on a three-year course in Total Technology unless they are convinced of the value of the course. So, said Sir Brian, proposals for courses and doctorates in Total Technology should, at first, be implemented on a pilot scale until industry's reaction has been assessed and the value of the scheme established.

The Engineering Board's proposal for the PhD course in Total Technology is that the course be made up of three overlapping phases. The first would consist of intensive course work on the chief sectors of industry, with formal courses, studies of the operations of small companies and case studies.

The second phase would consist of courses on background subjects such as design, economics, production technology, plant maintenance and marketing as well as specialized courses in advanced technology to prepare students for the third phase, which would be a project.

The emphasis in the course would be on real industrial situations discussed in tutorials and simulated in the laboratory. This combination of studies, emphasized Sir Brian, will remove any danger of the course being shallow—a fear which is generally expressed about broad based PhD courses. A PhD based on such a course, said Sir Brian, would be just as much, if not more, of an intellectual challenge to the best engineering student as a conventional PhD course.

SOVIET SCIENCE

Glaciers Studied

from our Soviet Correspondent

FOR more than 40 years, Soviet glaciologists have maintained a permanent observation station on the Fedchenko Glacier in the High Pamirs. Press coverage (*Izvestiya*, September 27, 1972) of the servicing of this station by helicopter reflects increasing concern as to how this large glaciated area (several thousands of square km) affects the climate, and how water from these glaciers can be used for the irrigation of Soviet Central Asia.

A helicopter exploration expedition carried out in 1971 established the existence of eleven previously unknown glaciers in the High Pamirs which feed the Vartang basin. The expedition, however, used helicopters only for aerial survey, and no attempt was made to land.

In June 1971, V. A. Litosh, a young geologist at the Kh. N. Abdullaev Institute of Geology and Geophysics in Tashkent, working on data of glacio-

Computer Posts

THREE new members have been appointed to the Computer Board. The Department of Education and Science announced recently the appointment of Professor A. J. Brown, Professor of Economics at the University of Leeds, Professor H. H. Rosenbrock, Professor of Control Engineering at the University of Manchester Institute of Science and Technology, and Professor N. Wells, Professor of Computing Science and Director of Computing Services at the University of Leeds, to serve until July 1974. The board, which was created in 1966, advises the government on the provision of computers at universities and guides the research councils in their use of computing.

Professor Brown will represent the University Grants Committee on the board. Professor D. J. Finney, Professor of Statistics at the University of Edinburgh, and a member of the board since its inception, has been appointed chairman, also until July 1974.

logical expeditions of the Academy of Sciences of the Uzbek SSR, established a model which predicts that the ice in the Pamir and Altai glaciers changes completely in less than 1,000 years. The model also predicts that the rate of shrinkage of these glaciers has been about 0.8 per cent every year during the past 25 years.

It is still the Fedchenko glacier (named after the leader of the first scientific expedition to the Pamirs in 1871), however, which is the chief target of research. Over 70 km long, 3-4 km wide and up to 1 km thick, its 500 cubic kilometres of ice account for one third of the ice content of the entire Pamir glacier system. The Fedchenko research base, at 4,200 m above sea level, works in close conjunction with the Altyn Mazar weather station several km away. It is permanently manned by five to seven people, who make round-the-clock observations of the glacier itself and also of the weather conditions and solar radiation, including ultraviolet and shortwave emission.

TECHNOLOGY

High Risk Research

THE government should drop its involvement in civil high technology, according to Professor John Jewkes, Professor Emeritus in Economics at the University of Oxford. Delivering the Harold Wincott Lecture last week, Professor Jewkes lambasted the government's involvement in high technology which, he said, "is due to an almost neurotic reaction to the fear that other countries are treading on our heels or have indeed actually surpassed us".

Professor Jewkes went on to argue that the only thing that is "high" about high technology is the level of risk involved. High technology is technology that private companies will not finance because they cannot see a reasonable return on their investment; "governments for one reason or another feel that they know better".

Professor Jewkes also suggested that in high technology, the government cannot learn from its mistakes, because each set of errors is unpredictable—the same things are not going to go wrong each time.

So, Professor Jewkes argued, might the government not do better to stick to financing science in the universities and maintaining research establishments on beneficial services such as public health, metrology, and transport safety? If the government did face up to "their falling standards of success and decided that activities with lower priorities should be dropped, surely involvement in civil high technology should be regarded as one of the first for the discard".

POLLUTION CONTROL

Alkali Report

THE public must be given more information about airborne pollutants from factories, according to Mr Eldon Griffiths, under-secretary of state at the Department of the Environment. Speaking at the publication of the Alkali Inspectorate's annual report this week, Mr Griffiths added that "confidentiality must now come to an end and industry and government must make much more information available to the public".

Mr Frank Ireland, the chief alkali inspector, echoes Mr Griffiths's sentiments, although tacking a few conditions on the end. The Alkali Inspectorate is responsible for ensuring that the emissions from the 1,875 works registered with the inspectorate under the Alkali and Works Acts and Orders come up to standard. The inspectorate consists of 36 inspectors organized into 15 districts, and whereas Mr Ireland is eager "to have more information given to the public about processes and emissions" he is not willing to encourage the public to telephone his inspectorate to learn about emission levels from particular factories. He sees the task of educating numerous enquirers in the meaning of emission data as a "non-productive" task for the inspectorate. Local liaison committees however, on which the inspectorate, local industry, and the public are represented, and of which there are now fifteen, are seen as valuable.

The inspectorate's work was considerably broadened in 1971, when acrylates, di-isocyanates and mineral works came under its purview, and the inspectorate has some fairly hard things to say about the mineral works. While the best are "excellently chemically engineered", the report states that "the majority are dusty, uncoordinated pro-

cesses with bad housekeeping and inadequate arrestment or containment. Best practicable means (of emission control) for some is to pray for rain during dry weather". Mr Ireland is however, encouraged by the industry's willingness to cooperate with the inspectorate.

The inspectorate's policy of cooperation with industry to achieve pollution control has been criticized recently, and Mr Ireland is clearly very sensitive about it. "Cooperation is much better than coercion" he said, while Mr Griffiths pointed out that the inspectorate's achievements are the envy of other countries. But Mr Ireland is willing to admit that the penalties behind his inspectorate are "rather derisory". The maximum fine under the Alkali Act is £100, and the highest fine imposed under the three successful convictions brought last year was only £65. Stiffer measures are, however, under consideration.

The annual report also reveals that the UK Atomic Energy Authority has been having trouble with the activated carbon beds fitted to its nuclear reactors to absorb radioactive iodine which might be released under emergency conditions. One station was found to have poor absorption when tested *in situ* and was subsequently replaced. The bed is now 99.99 per cent efficient, and the fault was found to be in the sealing round the filters and not in the carbon itself. But the problem of imperfect sealing is a difficult one which has previously caused problems with particulate filters. The CEBG is now testing all the particulate filters at its existing power stations.

The report, which covers Scotland as well as England and Wales, presents a detailed account of the inspectorate's work and lists instances where there have been infractions of the standards set by the inspectorate.

ANTI-POLLUTION

In Trafalgar Square

LIKE a canvas and scaffolding environmental disaster, the British Environmental Exhibition is now squatting in Trafalgar Square. The triangular geodesically patterned dome, which first saw the light of day at Stockholm, is a curved maze of photographs, slides and exhibits displaying with some pride the great strides made by the British in improving their environment. Touchingly bilingual, the exhibition sports pictures of London and the north before and after the Clean Air Act, and a list of British legislation on the environment. Garden cities are given their perennial puff, and spaghetti junction (Gravelly Hill motorway inter-

change) is presented as one of the greater achievements. Writing on the walls tells the inquisitive that as the pioneer of the Industrial Revolution, Britain was also the pioneer of pollution (and now of pollution control), while a six-screen, twelve-projector, audio-visual colour presentation (with that bilingual commentary in case the French are over) presents environmental triumphs from waste disposal upwards. Enquiry cards that can be despatched to the Department of the Environment for official response are littered in the alcoves, along with glossy handouts to take away and read (or take away and throw in Trafalgar Square's fountains, as some environmentally conscious members of the public have done). The question to ask is what it is all about?

NEW WORLD

Living with Uncertainty

by our Washington Correspondent

Now that the dust has settled after President Nixon's second veto of the appropriations bill for the Departments of Labor and of Health, Education and Welfare, some officials at the National Institutes of Health are predicting that the veto will not greatly harm their activities, provided they get their final budget sorted out early in the New Year; others are not so optimistic, however, and there is an air of gloom in some quarters over the fact that the institutes will have to live with a small budget at least until the fiscal year is more than half way through. There is little rejoicing in the National Heart and Lung Institute, for example.

On the face of things the National Cancer Institute should be the hardest hit in the NIH by the veto simply because it was set for the biggest increase in both the Administration's budget and in the vetoed bill. Until Congress passes a new bill which is acceptable to the Administration, the NCI will have to be content with the same amount of money as last year—some \$378 million. The Administration's budget for the present fiscal year would have given the institute \$432 million, while the vetoed bill set the budget at \$492 million.

Two factors will help the NCI to survive relatively comfortably until its 1973 budget is sorted out. The first is that the institute received a large budget increase last year which allowed it to start up new programmes and increase fundings of others, so that even at the 1972 budget level the institute cannot complain that it is starved of money. The second is the fact that the institute received a supplemental appropriation of some \$40 million near the end of the 1972 fiscal year. This has reduced the difference between last year's budget level and the Administration's request for this year from about \$90 million (the figure mentioned in last week's article, see *Nature*, 240, 8; 1972) to \$54 million.

Officials at the cancer institute are also hopeful that the effect of the veto will be minimized by the fact that most of their grants and contracts are let during the second half of the fiscal year, when the final budget figures should be available. Mr Calvin Baldwin, Executive Officer of the institute, said last week that he and his colleagues are planning on the assumption that they will finally get at least the money which the Administration requested, so

that although they can now only spend up to the 1972 level, they will have other contracts almost ready to sign as soon as the new appropriations are cleared.

The optimism in the NCI is clearly contingent on the assumption that the budget will be sorted out early in the new year, so that the letting of grants and contracts will not be too badly delayed. It is unlikely, however, that Congress will be able to get a fresh bill out and signed by the President much before mid-February, by which time

more than seven months of the 1973 fiscal year will have gone by.

As for the National Heart and Lung Institute, the Presidential veto will bite fairly deeply because the crusade against diseases of the heart, lungs and blood vessels is lagging about a year behind the crusade against cancer. The NHLI, unlike the National Cancer Institute, does not have a large 1972 budget increase to help tide it over the period of uncertainty until the 1973 budget is worked out and it now has programmes awaiting funds. The 1972

INTERNATIONAL COOPERATION

Accord with Poland

by our Washington Correspondent

THE governments of the United States and Poland last week signed a broad agreement on cooperation in science and technology. Although it lacks a commitment from either government to spend money or to undertake definite programmes, the agreement is a significant development in the growing *rapprochement* between the United States and Eastern Europe on scientific affairs. It is also yet more fruit from President Nixon's globe-trotting during the past year.

The agreement provides for joint research and development projects between institutions in the two countries, the sharing of results and data from scientific studies, the exchange of scientists and other specialists between institutions and the organization of joint courses, symposia and conferences. In other words, the agreement broadens the scope of present cooperation in science and technology between the two countries, elevating it from the level of *ad hoc* arrangements between institutions to a formal agreement between the two governments. There is provision for periodic review "at a senior level" of activities carried out under the agreement but, unlike the science agreement between the United States and the Soviet Union, there is no provision for review and planning by a joint commission.

Cooperation between the United States and Poland on science and technology projects first started some ten years ago. Initially, three government agencies—the Departments of Agriculture and of Health, Education and Welfare and the Smithsonian Institution—were involved in collaborative projects,

chiefly in the fields of agriculture and medical sciences. But last year the Departments of the Interior and of Transportation, the National Science Foundation, the Environmental Protection Agency, the National Oceanic and Atmospheric Administration and the National Bureau of Standards joined the list of individual government agencies in the United States that are forging scientific links with Polish agencies on a host of projects. The US government spent some \$11 million on its share of the projects in the 1972 fiscal year—about double the expenditure level of previous years. Non-governmental scientific institutions and universities in both countries have also played their part in formal and informal cooperative ventures.

Whether these *ad hoc* ventures will be broadened and expanded by the agreement signed last week remains to be seen, but one effect of the new accord may be progress towards similar agreements between the United States and other East European governments. Apart from the major agreement between the United States and the Soviet Union, last week's treaty was the first to be signed at governmental level with an East European country. On July 7, however, an interagency agreement was signed between the National Science Foundation and the Institute for Cultural Relations of the Hungarian Peoples Republic, and the United States and Hungarian governments are in preliminary negotiations for an agreement encompassing science, technology, culture and trade.

Meanwhile, the first meeting of the joint US-USSR commission to plan and oversee the workings of the Moscow accord is now expected to take place in Washington some time in December. It was scheduled for October, but preliminary studies were not completed in time.

budget for the NHLI was \$232 million, the Administration's budget request for 1973 was for \$255 million and Congress had stepped in with a huge increase to \$320 million.

Like their colleagues at the National Cancer Institute, officials in the National Heart and Lung Institute are planning on the basis that they will receive at least the money requested in the Administration's budget. But the amount contained in the vetoed bill constitutes such a quantum leap from the present budget level that any euphoria arising from the increase earmarked in the Administration's budget will quickly be dampened if the institute does not also get at least some of the extra money voted by Congress.

Asked last week which programmes may be affected by the veto, Dr Robert Ringler, Deputy Director of the NHLI, suggested that research into diseases of the lungs and new efforts directed towards ascertaining risk factors associated with heart disease may not receive the early financial boost that was planned for them. NHLI took over responsibility for lung research in 1969, and Dr Ringler said that he believes that area of the institute's activities has not been able to grow rapidly enough.

As far as the National Institutes of Health as a whole are concerned, NIH officials are now involved in negotiations with the Office of Management and Budget for the 1974 budget, uncertain of the funding level for 1973. They can be sure of one thing, however. With the Administration fixed on its present course to hold down federal spending, OMB will probably not realize much more money for 1973 than the Administration requested—whatever Congress decides.

HUMAN EXPERIMENTATION

Ethics in Public

by our Washington Correspondent

SENATOR KENNEDY's amendment to the Defense Appropriations Bill, which is designed to force the Department of Defense to draw up fresh guidelines for research involving human subjects (see *Nature*, **239**, 363; 1972), has survived scrutiny by a conference committee, been cleared by Congress and will become law when the bill receives President Nixon's signature. Flushed with the success of that minor foray into the field of medical ethics, Kennedy announced recently that his health subcommittee intends to hold hearings into human experimentation early next session, and that he has instructed his staff to seek out any evidence of experiments which have a dubious ethical base.

What seems to have pushed Senator Kennedy into announcing the hearings

so far in advance is the disclosure of a study of the effects of prophylactic treatment of premature babies with the antibiotic chloramphenicol, which was conducted at Los Angeles County Hospital in 1958. According to an article to be published in *World* magazine, there was considerable doubt about the safety of the drug in the early 1950s, although by the end of the decade it was still being sold in large quantities. The study was therefore designed to establish whether the survival rate for premature babies was increased or diminished by the drug.

The study came up with clear evidence that chloramphenicol used as a prophylactic greatly increased the mortality rate in premature babies when compared with penicillin or even no drug at all, and the study played a large part in the drastic reduction of use of the drug in nurseries.

According to Kennedy, one of the chief doubts about the experiment is that it involved chiefly the children of poor uneducated Americans, and he also wondered why the drug was still in use twenty years after its dangers had been brought to light. According to a doctor involved in the study, however, it was then standard practice to give antibiotics to premature babies as prophylactics, and by causing chloramphenicol to be virtually taken out of such usage, the research was valuable.

FEDERAL SPENDING

Two More Casualties

by our Washington Correspondent

LEGISLATION dealing with science and technology figures prominently among the casualties of President Nixon's campaign to hold down federal spending. Last week, after having refused to sign the appropriations bill for the Department of Health, Education and Welfare, he vetoed legislation designed to set up within the National Institutes of Health an institute for research on ageing and declined to put his signature to a bill which would have substantially increased federal support for minerals research. Both bills were classed in the category of "budget wreckers" by Nixon, although he said that an added reason for turning down the minerals research bill was that it would have fragmented federal programmes in that field. Congress had no chance to override his vetoes because they came well after the adjournment.

The proposed National Institute on Aging (see *Nature*, **239**, 425; 1972) would have been the focal point of the federal government's biomedical research programme into the ageing process and it would also have been concerned with the psychological and sociological problems associated with growing old. Sponsored by Senator

Short Notes

A Slight Delay

Two technical hitches have delayed the launch of NASA's gamma ray astronomy satellite, SAS-B. The launch, which was set to take place on November 2 from the San Marco range off the coast of Kenya, is now expected to take place on November 16. One problem was with the Scout rocket, in which the gyro did not function properly; it was replaced with a spare on site. The second problem was a bad solder joint in the telemetry encoder aboard the spacecraft. The unit had to be flown back to the United States for repairs, but it is now undergoing final checks in Kenya.

SAS-B will be the first satellite devoted entirely to gamma-ray astronomy since the small and relatively unsophisticated Explorer II.

Erratum

The item last week in Short Notes under the heading "Kennedy Bill" (*Nature*, **240**, 8; 1972) was included by mistake—the hearings referred to were in fact held in September. The Editor apologizes for any confusion this may have caused.

Tom Eagleton, the bill was rushed through Congress shortly before it adjourned, but the legislation did not contain any budget figure for the proposed institute—that would have been determined by the appropriations committees after review of how much of the work of other institutes in NIH would have been taken over by the new institute. The bill was a direct result of recommendations from the recent White House Conference on Aging, and Mr McGovern lost no time in castigating President Nixon for reneging on his promise to implement the recommendations from that conference.

The other casualty, the bill designed to aid minerals research, would have provided the Department of the Interior with money to finance research on minerals and mining technology at academic and industrial institutions. Some of the money would also have gone towards setting up research centres at state-supported universities and colleges. The bill authorized expenditures of \$25.2 million in the 1973 fiscal year, increasing to about \$40 million in five years' time. Both bills enjoyed strong bipartisan support during their passage through the Congressional mill, and will probably reappear next year. They should fare better outside the atmosphere of election-year politics.

NEWS AND VIEWS

How Much of IQ is Inherited?

FOR several years, educational psychologists have been arguing among themselves and with a wider public about the extent to which measured IQ is determined by genetic rather than environmental considerations and, in particular, about the contentious issue of whether the evidence for genetic inheritance is convincing enough to provide support for hypotheses that different races differ genetically in IQ and whether there may even be such differences between different social groups within the same race. At times, the argument has become so fierce that a moratorium on investigations of this kind has been suggested. More recently (see *Nature*, **239**, 1; 1972), a distinguished group of scientists has supported Professor A. R. Jensen in his plea that those who, like himself, wish to pursue investigations in this field should not be pilloried as racialists, and that is entirely proper. It would be outrageous if sober scientific investigations were halted simply because some of the social implications might improbably be awkward. But the subject is so contentious that those working in the field should be more than ordinarily aware of the public impact of the publication of their research. First, they have a duty to ensure that they do not themselves erect unfounded generalizations on the data which are available. Second, in these exceptional circumstances, there is a case for saying that any strictly scientific article should be accompanied by a warning that its conclusions, however valid, are likely to be irrelevant to the making of social policy. Even if somebody were able, for example, to demonstrate genetic differences of the mean IQ of whites and blacks in the United States, and to many minds this remains an open question, it would be proper if the authors of that hypothetical research were to acknowledge that their conclusions need not divert policy makers from the liberal courses on which they have mercifully embarked. This is a point on which Professor Jensen has been criticized.

The article by Drs L. J. Eaves and J. L. Jinks in this issue of *Nature* (see page 84, necessarily highly technical, amounts to a fierce criticism of an article by Dr Sandra Scarr-Salapatek (*Science*, **174**, 1285; 1971). The issues are those of the suitability of data and the interpretation put on them. Dr Scarr-Salapatek was primarily concerned to disentangle two hypotheses for the observed differences of mean IQ between different social groups—the hypothesis that the differences are to be accounted for environmentally and the hypothesis that social groups of a distinctive mean IQ differ in their genotypic distribution from each other. This is the old argument between nature and nurture, but with the reservation that the terms are more precisely defined than they used to be and with the expectation that both environment and inheritance are likely to play a part in the determination of mean IQ. The question is how much.

One of the best ways of throwing light on problems like these is by the study of pairs of monozygous and dizygous twins reared together. Two years ago, Jinks and Fuller

(*Psychol. Bull.*, **73**, 311; 1970) published the results of such an analysis of IQ data whose general conclusion was that the relative contributions of inheritance and environment could not be disentangled. There have also been at least four important studies of the mature IQ of monozygous twins reared separately, the best known of which is that of Burt (*Brit. J. Psychol.*, **57**, 137; 1966)—these are ways of estimating the degree to which genetic components of IQ persist in a variety of environments, but data are necessarily hard to come by. What Dr Scarr-Salapatek attempted to do, in her article last year, was to quarry data for very large numbers of monozygous and dizygous twins from the Philadelphia school registers in April 1968. Twins were defined as pairs of children with the same surname, the same birthday and the same home address. School records are not, of course, an unambiguous way of identifying monozygous twins; it is possible to estimate the frequency of monozygosity from the frequency of pairs of apparent twins of the same sex in the sample as a whole, but there is necessarily a loss of precision in estimating statistics such as the ratio of genetic variation within families to the total variation arising from all sources within families. One of the criticisms which Eaves and Jinks now make of Dr Scarr-Salapatek's argument is that the way in which the Philadelphia data have been assembled makes it impossible to estimate correlations between genotypic and environmental influences on measured IQ except by making assumptions for which there is no *a priori* justification.

But Eaves and Jinks has also important criticisms to make of the estimates of significance of Dr Scarr-Salapatek's statistical parameters which themselves derive from the indirect identification of monozygosity. Indeed, they conclude nearly 4,000 pairs of twins would be needed to provide a 95 per cent chance of detecting even gross differences between the heritability of a trait in the black and white sections of the population of the Philadelphia schools, nearly three times as big as the sample actually used. Indeed, Eaves and Jinks go on to argue that the Philadelphia sample is too small even to support the well-established conclusion that individual differences in intelligence are at least in part genetic in origin. Certainly, according to Eaves and Jinks, the data from Philadelphia are entirely insufficient to support conclusions about the heritability of IQ in different socioeconomic classes as well as between the white and black populations sending children to the Philadelphia public schools—one of Dr Scarr-Salapatek's innovations. In short, their argument is a vivid reminder that in this contentious field, although it would be entirely wrong that constructive argument and analysis should be crabbed, there are powerful reasons why those working in the field should bend over backwards to ensure that inadequate data are not used as a foundation for conclusions which turn out to be statistically and even methodologically unsupportable.

Lunar Alkali Metal Poverty

ONE of the more remarkable features of the basalts returned from the Sea of Tranquillity in July 1969 was their depletion in sodium, potassium and rubidium relative to otherwise comparable basalt on Earth. Factors of order 10^{-1} are involved and the depletion has proved to be a general feature of mare basalts; other lunar rocks also contain relatively small amounts of, particularly, sodium, although obviously comparable terrestrial rock types may not exist.

Two working hypotheses to explain this poverty in alkali metals immediately suggest themselves, and both have vigorous schools of adherents at the present time. The first maintains that the observed abundance is a primitive feature of lunar chemistry, with the implication that the interior of the Moon is quite different from, say, the Earth's mantle or mantle plus crust. If one assumes that the mare basalts were derived by partial melting of the lunar interior and were erupted without undergoing further fractionation of solids or vapours from the liquid, then experimental reproductions of the process show that they cannot, indeed, be derived from an Earth-like mantle rich in the refractory phase olivine; instead they must be derived from a source region rich in clinopyroxene. This is, in essence, the working hypothesis adopted by Ringwood, Green, and their colleagues at the Australian National University, Canberra. It has the innate attractions of being a simple model and implying that one of the theories advocated for the origin of the Moon—fission from the Earth (during formation of the core?)—is untenable. Further, this straightforward hypothesis has received strong support from work on the rubidium-strontium chemistry of the mare basalts, which suggests that, with minor exceptions, the basalts have been closed systems in respect of their Rb: Sr ratio since 4.6×10^9 yr ago (about the time that the Earth and meteorites condensed in the solar system) in spite of the fact that their present mineral assemblages crystallized at various dates around 3.5×10^9 yr ago. The most straightforward interpretation here is that the mare basalts were melted from a somewhat inhomogeneous primitive lunar interior, without any further fractionation, during the events at $\sim 3.5 \times 10^9$ BP.

The development of the second working hypothesis to explain the apparent lack of alkalis on the Moon has been, for the most part, in the hands of O'Hara at Edinburgh University. This model assumes a petrological equivalent of Murphy's law: that if a fractionation process is possible then it is almost bound to occur. Certainly igneous rock sequences on Earth provide abundant evidence in support of this view. O'Hara and his colleagues have maintained, as a result of their melting experiments on lunar mare basalts, that crystal-liquid fractionation must have occurred under conditions close to those on the surface, because the basaltic liquids were saturated, or nearly saturated, on eruption with the mineral phases they crystallize. Alkali metal depletion is explained as a consequence of fractionation into a vapour phase, which then escaped from the Moon's gravitational influence. Experiments show that molten rocks do, indeed, lose alkalis to a low pressure vapour phase; whether the vapour could then have escaped is a more difficult question set about with a plethora of unquantified parameters. But the

probability of escape is dominated by terms in mv^2 . By happy coincidence the ratios of squared escape velocity from Earth and Moon, and the weights of sodium and hydrogen, are almost the same, so, other things being equal, the loss of hydrogen from the Earth and the loss of sodium from the Moon have similar probability. Thus the model is not *prima facie* ridiculous.

A consequence of the complex process model is that the chemistry of the Moon's source region for basalts cannot be determined from an inspection of the basalts. Thus the interior of the Moon could be similar in constitution to the Earth's mantle. Indeed, it is probably easier for most petrologists to view O'Hara's model as springing from the premise that the Moon's interior is similar to the Earth's mantle.

So far, it can be seen that both models are lent some support by the petrochemistry of mare basalts, although proponents of both have to do some rather tricky arguing to explain away the stronger points of the other: this is seldom an insuperable obstacle to petrological theory.

Last year, Brown and Peckett (*Nature*, **234**, 262; 1971) reported analytical work on the feldspars of the basalt 14310 which seemed to confirm that sodium and potassium had, indeed, fractionated into a vapour phase during crystallization. Now 14310 is not a mare basalt of the type returned by Apollo 11, 12 and Luna 16; it comes from the Fra Mauro formation, thought to consist of debris thrown away from the site of Mare Imbrium during the formation of that sea 3.9×10^9 yr ago. It may have crystallized from a purely local melt at 3.9×10^9 yr or it could conceivably have survived intact from the 4.3×10^9 yr old pre-Imbrium event of O'Nions and Pankhurst (*Nature*, **237**, 446; 1972). Unlike the mare basalts, 14310 is highly aluminous, a feature which is expressed in the abundance of plagioclase in the rock, both as phenocrysts formed early on and as small, rapidly crystallized laths in the groundmass. Brown and Peckett demonstrated compositional zoning of the phenocrysts from cores with up to 95 per cent of the relatively refractory $\text{CaAl}_2\text{Si}_2\text{O}_8$ end member to rims enriched in the $\text{NaAlSi}_3\text{O}_8$ end member (75 per cent $\text{CaAl}_2\text{Si}_2\text{O}_8$) as might be expected during crystal-liquid fractionation. The groundmass crystals had compositions similar to the phenocryst cores, however, suggesting sodium loss from the liquid after formation of the phenocrysts and presumably on eruption.

The above interpretation was questioned by Green, Ware and Hibberson (*Nature*, **238**, 450; 1972), who doubted the significance of the sodic phenocryst rims, preferring to regard these as late overgrowths, and found the similarity of phenocryst cores and groundmass compositions perfectly compatible with crystallization in a closed system.

On page 95 of this issue of *Nature*, O'Hara has returned to the fray with a defence of the original interpretation of 14310 which introduces the further concept that water must have been involved in the production of such an aluminous melt in the first place: the water, too, has volatilized. Whatever is the case for 14310 (a basalt of problematic field relations: never the easiest situation for definitive interpretation) the idea of wet melting has far reaching implications. Plagioclase is much more soluble in wet melts than dry and, if the pre-mare differen-

tiation of the Moon is regarded as having involved wet melts, then it is reasonable to interpret the plagioclase-rich lunar highlands as precipitates from the drying out of these melts as they were exposed to the low pressure atmosphere.

The study of lunar petrology is still very much in its infancy and, to the outside observer, it seems that nothing but good can come from continuing to explore the consequences of as many working hypotheses as possible at this stage.—S. W. R.

Problems Still with Scrapie Agent

In working with the class of agents which cause scrapie, transmissible mink encephalopathy, kuru and Creutzfeld-Jacob disease there have been two recurrent problems. These are the difficulty of distinguishing primary from secondary events in the disease and the need to use whole-animal assays, which last many months or even years, in order to detect and titrate the infective agents.

Scrapie agent has now replicated in a cell culture line for more than 100 tissue culture passages, but without any signs of its presence, so this has not provided any basis for a quicker assay of infectivity. Equally frustrating have been the attempts to develop immune assays specific for infectivity, although Gardiner showed six years ago that scrapie tissues have increased antigenicity. The average concentration of infective units of scrapie in brain is ultimately higher than elsewhere but only reaches about the same order as the number of brain cells, and it has not been possible to purify and concentrate the agent. This is generally regarded as one reason why many workers have failed to detect a specific immune response, for the "antigen" has been a more or less crude preparation of tissues from affected hosts. Such tissues presumably contain many antigens associated with the chronic degenerative changes in the brain and, even with the infective tissues outside the central nervous system, the failure to observe tissue damage may simply reflect the insensitivity of present techniques. On the biochemical side, the inability to differentiate between the primary aspects of agent replication and the secondary tissue changes has produced a decade of studies, the results of which are mostly equivocal.

Within a month, two entirely different methods have been published which may reduce to a few days the time needed for assays in this context. It is therefore opportune to discuss their likely merits in the light of the types of difficulty which have beset work with scrapie. Carp, Licursi, Merz and Merz (*J. Exp. Med.*, **136**, 618; 1972; and *Infect. Immunity*, **6**, 370; 1972) used both multiple sclerosis and scrapie, and they report a replicating factor which is first detectable in mice by a reduction in the proportion of circulating polymorphs the day after injection with tissues from cases of multiple sclerosis. A similar effect was found to occur three days after injection with scrapie tissues. With both scrapie and multiple sclerosis tissues the effect persists at least for many months.

The other report is on page 104 of this issue of *Nature*. Field and Shenton have used a lymphocyte sensitization technique which differentiates efficiently between tissues from uninfected mice and those from mice affected by scrapie: the method involves a combined *in vivo* and *in vitro* test which takes about a week. It will be a real advance if both these reports fulfil the expectations which they will generate.

The most exciting possibilities come from the work of Carp and associates, because they may not only provide a rapid assay but may also bridge the gap between scrapie-like diseases and multiple sclerosis. The confidence of their report, with its modest claims for the assay, rests on the finding of a consistent difference between tissues from normal and affected subjects in the ability to produce the polymorph reduction. A number of different tissues were used, which came from several human, sheep and mouse sources. The authors are careful to state that the "virus" in the mouse assay may well be unrelated to the aetiology of multiple sclerosis, and in the case of scrapie they only conclude that it may be related to scrapie agent in some way, even though the assay virus seems to have some properties in common with scrapie agent. This is prudent because they have not yet produced evidence to fulfil the criteria which earlier workers with scrapie found necessary even before it was easy to work with scrapie in laboratory animals (Mackay, Smith and Stamp, *Vet. Rec.*, **72**, 1002; 1960). The high thermal stability of scrapie permits serial passage using boiled inocula each time: this eliminates from the inocula conventional viruses which could be producing misleading effects. Even though the polymorph assay may be specific for scrapie, multiple sclerosis and any related diseases, it is important to know if the effect comes from a non-causal secondary virus normally present in such diseases. Another conceivable possibility is that multiple sclerosis and scrapie tissues merely release a latent virus in the mice, which would not necessarily invalidate the assay though it would be less interesting: this alternative is perhaps excluded by the authors' statement that "the characteristics of the multiple sclerosis and scrapie factors that induce the decrease are similar but not identical". Their findings will need independent confirmation and may not prove to be an assay of the respective causal agents, but the prospects are that this approach will not incur problems of dealing with effects merely due to tissue damage.

In the case of the lymphocyte sensitization test it is to be hoped that the claim of Field and Shenton, that it "enables the presence of scrapie to be established within 8 days" rather than several months, will prove correct. As the evidence stands, however, this is too broad a generalization and other kinds of evidence are needed. The problem is to find a way of distinguishing between the agent and tissue damage antigens, and the authors are aware of this difficulty. The scrapie brain and spleen tissues so far tested presumably come from mice at an advanced stage of the disease, and chronic degenerative changes will have been present in the brain for many weeks. It cannot be assumed that the spleens of such animals have not been chronically exposed to any changed antigens arising in the brain. If the test is eventually shown only to detect tissue damage as an index of agent

concentration it will still be useful because it is "exquisitely responsive to minor changes in antigenic determinant structure" and can evidently detect more subtle degrees of damage than can at present be quickly estimated by conventional histological means. Some of these possible secondary effects could be excluded by using it for a time sequence study of spleen in the four weeks after injection, when the concentration of the agent is rapidly increasing there but long before gross signs of damage appear in the brain, or, better, to compare the new and conventional assays using the tissue culture line developed by Haig and Clarke (*Nature*, **234**, 106; 1971).

But even in these cases the difficulty remains because of the high sensitivity of their technique, which may be detecting antigens from unseen damage. Its value will only be established when it can be shown that there is a close correlation between the titre estimates of the conventional assay and this new one in a large range of circumstances. This range will need to include various severe physical and chemical treatments of tissue preparations, which could alter the antigenic spectrum without removing all the infectivity. If a close correlation cannot be established between the two types of assay, it will be necessary to await the purification of scrapie as an antigen.

It may therefore be premature to use the method for collateral evidence that there is more scrapie agent in neuronal than glial preparations, as is done in the accompanying communication by Narang, Shenton, Giorgi and Field (page 106). At this stage standard evidence of infectivity titre is also needed for these two cell populations, and this would give further confidence in the use of the proposed assay. Meanwhile it is difficult to assess the significance of their electron micrographs of particles, but the detection of these in serial passages using only boiled inocula would be more impressive than other evidence.

As the lymphocyte sensitization assay takes only a few days it should be possible to deal quite quickly with some of the points raised here, even though, as Field describes elsewhere, the technique involves "a most fickle and unstable instrument which makes great demands upon the patience and endurance of the observer".—From a Correspondent.

PROTEINS

Further Phosphorylations

from our Molecular Biology Correspondent
THE role of phosphorylation in the control of glycogen metabolism, by way of cyclic AMP-dependent protein kinase, phosphorylase kinase and glycogen phosphorylases *a* and *b*, has by now been absorbed into the canon of biochemistry. There is now the glimmer of an intimation that phosphorylation may also take another role in muscle, related in fact to the contractile process.

The discovery that troponin, which is a part of the calcium-driven relaxing system of muscle, is prone to phosphorylation by endogenous enzymes has been reported simultaneously from two laboratories. Pratje and Heilmeyer (*FEBS Lett.*, **27**, 89; 1972) have prepared the relaxing protein mixture from myofibrils and observed the incorporation of radioactive phosphate from γ -labelled ATP when cyclic AMP was present. The relaxing system contains the helical protein tropomyosin and troponin. The latter, as is now known, contains three different chains, of defined function: the inhibitory component (TNI) inhibits actomyosin ATPase; the second, the calcium-binding component (TNC), reverses the inhibition regardless of whether or not calcium is actually bound; and the third (TNT), when combined with the other two, restores the control of the actomyosin ATPase by calcium ions, in that it apparently governs the binding

of the inhibitory chains to tropomyosin.

The three proteins differ in molecular weight, and are therefore separable in SDS-acrylamide gel electrophoresis. Phosphorylation proceeds slowly in this system under the stimulus of cyclic AMP, because of the presence of traces of protein kinase. When purified protein kinase is added to the mixture, the reaction goes rapidly, and the location of label in the electrophoresis gels after incubation shows that actin and one of the troponin components only are appreciably phosphorylated. This is the third, and highest molecular weight, chain (TNT). There is at this stage no indication that the phosphorylation changes the relaxing activity of the system *in vitro*, and the control may be exerted at an earlier stage, in making the phosphorylated component more or less available where it is wanted.

Meanwhile Krebs and his colleagues have looked for, and demonstrated, phosphorylation of troponin under the action of the (in general) much more specific enzyme phosphorylase kinase, the only good substrate for which hitherto known was glycogen phosphorylase *b*. Stull, Brostrom and Krebs (*J. Biol. Chem.*, **247**, 5272; 1972) now find that in the presence of ATP and magnesium the enzyme causes phosphorylation—again of only one of the troponin chains, the inhibitory component. Moreover, the enzyme works in both its forms, activated and unactivated (that is to say, whether it is itself phosphorylated by protein kinase

Gene Duplication in Proteins

IN next week's issue of *Nature New Biology* (November 15), Kretsinger gives evidence of internal duplication of parts of the polypeptide chain of the fish muscle calcium-binding protein, the structure of which was recently solved at high resolution, and McLachlan considers the nature of, and possible tests for, such phenomena. Gene duplication within a single chain has been reported in a number of cases. In the calcium-binding protein, mere inspection of the sequence reveals no obvious homologies. There are two calcium-binding sites, with similar ligands for the ion, but which are not obviously related in sequence. Kretsinger finds, however, that if the three-dimensional structures in these regions are superimposed, the geometrical similarity becomes obvious. Even the backbone carbonyl ligand to the calcium, which is provided in one case by a phenylalanine and in the other by a lysine residue, falls into place. As in other proteins of known structure, such as staphylococcal nuclease and thermolysin, the calcium-binding mode is un-

related to that of the muscle protein, the argument against converging evolution is very strong. Thus it seems that structural homologies may transcend divergence of sequences. It also suggests that more versatile methods of searching for sequence homologies are needed, and this is the subject of McLachlan's communication. Using a scoring system for the degree of similarity of the various amino-acids, a homology in the two regions of the calcium-binding protein emerged, and McLachlan has also devised a way of testing for geometrical homologies in terms of the α carbon coordinates along the chain. Kretsinger suggests that a third region in the molecule, which is not implicated in the binding of calcium, is structurally related to the two calcium-binding segments, but McLachlan regards this as less certain. It is suggested that hitherto unsuspected structural homologies, which are only weakly reflected in the sequences (unless a sensitive test is applied), may eventually be shown to be present in many proteins.

or not) in terms of its activity towards glycogen phosphorylase. Radioactivity in SDS gels appears in the TNI zone, to a maximum extent of 0.4 mol phosphate per chain of TNI. In order to ascertain whether this limited uptake is a consequence of pre-existing unlabelled phosphate on the phosphorylation sites in a fraction of the molecules, total phosphorus was determined, and this explanation was borne out. When, moreover, all endogenous phosphate was stripped with phosphorylase phosphatase, and the resulting product was then rephosphorylated, uptake of one mol of phosphate per mol of protein was observed. The rate of phosphorylation of troponin by phosphorylase kinase was less by a factor of fifteen than that of phosphorylase *b*, but greater by 150 than that of casein (the only other substrate yet discovered).

The dephosphorylation by the specific phosphorylase phosphatase is also remarkable, and is the subject of an accompanying paper (England, Stull and Krebs, *ibid.*, 5275). The enzyme has never been isolated in visible form, and therefore England *et al.* have been at pains to demonstrate that it is indeed this same phosphatase which is responsible for the reaction in the crude preparation. They have shown that through the various stages of enrichment of the preparation the dephosphorylation of the troponin follows the specific activity of the enzyme towards phosphorylase *a*; that the activity towards both occurs in the same region of a polyacrylamide gel (though no protein band is visible on staining); that the activity is in both cases inhibited by fluoride; that the course of heat inactivation of the phosphate is identical in terms of both substrates; and that phosphorylase *a* and TNI compete as substrates. The Michaelis constant is somewhat greater on phosphorylate *a*, but the maximum velocity smaller.

It seems not at all unlikely, therefore, that inhibition of actomyosin ATPase by troponin is in some way modulated by its state of phosphorylation. As phosphorylase kinase is calcium-dependent, the release of calcium in the myofibril could have a secondary effect in terms of the phosphorylation of troponin. The cyclic-AMP dependent process is another matter, and it is the suggestion of Pratje and Heilmeyer that the effect of the hormone epinephrine on the rate of contraction of muscle proceeds by way of accumulation of cyclic AMP, which it is known to encompass, and thus of the phosphorylation of the troponin component TNI. It may be noted, however, that there was a report last year (Bailey and Villar-Palasi, *Fed. Proc.*, **30**, 1147; 1971) that it is TNI that is phosphorylated in response to cyclic-AMP stimu-

lation; Stull *et al.* mention in passing that they concur in this observation. This conflict therefore remains to be resolved.

GENETICS

A Two-edged Sword

from our Cell Biology Correspondent

"OUR goal," say Jackson, Symons and Berg at the beginning of their report in the current issue of the *Proceedings of the US National Academy of Sciences* (**69**, 2904; 1972), "is to develop a method by which new, functionally defined segments of genetic information can be introduced into mammalian cells", and if they use the vehicle they have so cleverly engineered there is a far from negligible possibility that they may end up achieving a great deal more than they seem to have bargained for.

Berg and his colleagues have, using the appropriate restriction endonuclease, terminal DNA transferase and DNA ligase, integrated a defined segment of the *Escherichia coli* chromosome, the entire *gal* operon carried as part of the circular genome of the transducing phage λ dv_{gal}, into the circular DNA of the tumour virus SV40. The resultant hybrid DNA molecule contains genetic information from three sources, namely a complete set of SV40 genes, some λ phage genes and the *E. coli gal* operon, and it may well be possible to infect and transform cells non-permissive for SV40 with this DNA. In such cells the SV40 moiety of the hybrid

molecule will presumably act as a vector for the insertion of the λ phage and *E. coli* DNAs into a chromosome(s), and several interesting questions concerning the expression of phage and bacterial genes in the foreign environment of animal cells can be answered unambiguously.

Both the rationale of these experiments and their execution, as far as it has been carried to date, are, of course, impeccable, but there is a fly in the ointment which no doubt Berg and his colleagues are well aware of and which may cause them misgivings about proceeding further and could result in their reagent being left permanently in the deep freeze.

The dilemma is obvious enough; if the SV40 moiety of this hybrid DNA can act as a vector for the insertion of the bacterial DNA into chromosomes of animal cells, then no doubt the bacterial and phage moieties can act as vectors for the insertion of the SV40 DNA into *E. coli* chromosomes. Such an event would, of course, tell us whether tumour virus genes are expressed in bacterial cells. A bonus to the experiment, perhaps, but *E. coli* and its relatives happen to constitute part of the human gut flora, SV40 transforms some human cells in culture and a virus virtually indistinguishable from SV40 has recently been isolated from the brain tissue of people suffering from multifocal leucoencephalopathy (Weiner *et al.*, *J. Virol.*, **10**, 147; 1972).

What would be the consequences if the reagent Berg and his colleagues have

Multiple DNA Polymerases in *B. subtilis*

THE biochemistry of DNA replication remains a rather murky subject but, as recent experiments have shown, in *Escherichia coli* at least seven genes specify products essential for DNA replication, and of the three DNA polymerases that have been identified to date only one, DNA polymerase III, probably has a role in replication. The biochemistry of DNA replication in the gram positive bacterium *B. subtilis* is even more obscure, but as the work reported by Neville and Brown and by Bazill and Gross in *Nature New Biology* next week (November 15) indicates that there are probably three sorts of DNA polymerase in this species, and that only one of them is a likely candidate for the task of semi-conservative RNA replication.

Neville and Brown have used the drug 6-(*p*-hydroxyphenylazo)-uracil (HPUra), which Brown's group have shown to inhibit DNA replication in *B. subtilis* cells before and after they have been rendered permeable by exposure to toluene, to investigate DNA polymerase activities in extracts of *B. subtilis* cells.

In summary their data indicate, first, that *B. subtilis* has more than one species of DNA polymerase and, second, that one species which is sensitive to inhibition by reduced HPUra *in vitro* may well be the enzyme involved in DNA replication *in vivo*.

Bazill and Gross, who have isolated mutant strains of *B. subtilis* that lack DNA polymerase I repair enzyme, have done somewhat similar experiments with extracts of these mutant as well as wild type cells. They have also compared the properties of the various *B. subtilis* DNA polymerase activities with those of *E. coli* DNA polymerases I, II and III. Like Neville and Brown, Bazill and Gross found two DNA polymerase activities in *B. subtilis* extracts as well as a repair enzyme activity. The most labile of these two activities was particularly sensitive to inhibition by HPUra and in other respects is reminiscent of *E. coli* DNA polymerase III. They also suggest that this activity may be involved in replication of *B. subtilis* DNA *in vivo*, a process specifically inhibited by HPUra.

made somehow infected and lysogenized *E. coli* in someone's gut as the result of an accident? This possibility, remote though it may seem to be, can hardly be ignored, and it will be most interesting to learn what criteria the group adopts when it decides whether or not the scientific information that might be obtained by continuing the experiment justifies the risk. Perhaps those involved will decide that the game is not worth the candle.

Whatever the outcome of any heart searchings the expertise with which the group have manufactured the hybrid DNA commands universal admiration. They have used the R_1 restriction endonuclease to convert circular SV40 DNA into linear molecules by cleavage at a unique site. They then removed a few bases from the 5' ends of the linear chains with λ exonuclease and added homodeoxypolymeric extensions (either poly (dT) or poly (dA)) to the 3' ends of the two chains of this linear SV40 DNA. By the same procedure they prepared linear λ dvgal DNA with 3' homodeoxypolymeric extensions. When the SV40 DNA and the λ dvgal DNA carrying complementary 3' extensions are annealed they form circular duplex molecules, the gaps and nicks in which were sealed with *E. coli* DNA polymerase I and DNA ligase to form covalently closed hybrid circular DNA molecules. And, of course, if this procedure is used with SV40 DNA alone the product is circular dimeric SV40 DNA.

As Berg and his colleagues observe, this procedure for covalently joining pairs of DNA molecules should be generally applicable with only minor modifications, and the exploitation of homopolymeric extensions means that quite unrelated DNAs can be joined. On the other hand these homopolymeric (dA.dT) tracts in the joined molecules may prove to be a hindrance when it comes to testing the biological activity of such hybrid DNAs, but that remains to be seen.

PERCEPTION

Size Constancy

from our Experimental Psychology Correspondent

Is adaptation to specific stimuli the psychologist's equivalent of the neurophysiologist's most successful of instruments, the microelectrode? If so, it offers one of the most important methods for approaching an explanation of perception in terms of the workings of the nervous system. It is appropriate therefore that in the new journal *Perception* (1, 111; 1972) one of the more interesting articles (by Blakemore, Garner and Sweet of the University of Cambridge) should describe an applica-

tion of this technique to the problem of size constancy.

The idea that neurones in sensory systems are each tuned to respond to a particular kind of event in the world is a very old one, going back at least to Descartes. More recently Sherrington described the event which a particular neurone seems designed to detect as its "adequate stimulus", and much of the success of modern neurophysiology can be attributed to the ability of the microelectrode to monitor the electrical activity of a single neurone; this provides an opportunity to discover its adequate stimulus. The discovery that cells in fairly peripheral parts of sensory systems are tuned not just to simple parameters such as sense modality or position on the body surface, but to fairly complex patterns, indicates that neuronal networks have an abstracting power which may be amenable to exact analysis. It further offers the possibility of explaining how the brain detects not just changes of energy on the receptor surface, but events of importance to the animal in the outside world. By continually showing an alert human subject a stimulus thought to be adequate and specific for a particular type of neurone, one can perhaps fatigue (that is, adapt) the same neurones from which one can record. This affords a means for investigating the activity of these cells and their effect on perception in man without recourse to

anaesthetics, surgery or any of the other encumbrances of physiological experimentation.

One perceptual effect of viewing an object in conditions under which its distance can be judged is that size constancy obtains; the object looks to be of a size appropriate to its existence in the real world, not a size dependent only on the extent of its retinal image. The size seen depends on the brain analysing not just its retinal size but also its distance. Adapting the observer to a sinusoidal grating stimulus (a set of stripes in which brightness varies sinusoidally across the grating), which Blakemore *et al.* argue is specifically detected by neurones involved in size estimation, might then give one of two results. Either such an adaptation would depress sensitivity in those neurones sensitive to a grating that width of stripe on the retina, or, if the neurones in question have already scaled the grating for its size in the real world, the depression of sensitivity would be in neurones sensitive to a particular apparent width of stripes. By adapting to stripes subtending a given angle at the retina, and testing with gratings at a different distance, Blakemore *et al.* found that the depression of sensitivity occurred for test gratings of the same retinal size as the adapting grating.

The effect is surprising because as one adapts to, say, a grating with wide stripes far away, it looks as if it has

Evidence of Human Cancer Viruses

IN *Nature New Biology* next week (November 15) two groups, those led by Spiegelman and by Gallo, report further experiments which they believe strongly support the idea that human RNA tumour viruses, analogous to the viruses which cause sarcomas and leukaemias in mice and chickens, not only exist but also play a part in the development of these diseases in man. Spiegelman's group have during the past several months used RNA/DNA hybridization techniques to probe for RNA molecules in human leukaemic cells which have homology with the RNA in mouse leukaemia virus particles. They believe they have found such RNA molecules and they now report that they have detected a complex of 70S RNA and RNA-dependent DNA polymerase in the cytoplasm of 95 per cent of the samples of human leukaemic cells which they have examined. Furthermore they found that when this complex was incubated with appropriate substrates, DNA was made which hybridized to the RNA from mouse leukaemia virus particles but failed to hybridize to the RNAs of mouse mammary tumour virus particles

and avian myeloblastosis virus particles.

Two of the diagnostic properties of the RNA tumour viruses of animals are the presence of reverse transcriptase and of 70S RNA, and so Spiegelman and his colleagues clearly think that they may have detected traces of a human leukaemia virus. Gallo and his colleagues also argue that they may be on the tracks of such viruses. They have found a reverse transcriptase activity associated with a particulate fraction from the cytoplasm of human leukaemic white blood cells. This activity, which has been partially purified, transcribes added avian myeloblastosis virus RNA into DNA, and because of its response to various synthetic RNA templates Gallo and his colleagues conclude that this activity is distinct from other cellular nucleic acid polymerases but is similar to the reverse transcriptase that occurs in animal and avian RNA tumour virus particles.

Obviously both these sets of experiments are consonant with the idea that human tumour virus particles exist, and they will no doubt stimulate further efforts to isolate these putative viruses in a pure and infectious form.

wide stripes. But the depression of sensitivity measured on a grating three times nearer corresponded to stripes which looked (and were) three times narrower. The authors conclude that neurones sensitive to spatial frequency are involved in estimating the size of visual stimuli, and that, in the visual system, these neurones operate before a mechanism which scales for size according to estimated distance.

This kind of conclusion does, of course, require an argument containing several more or less precarious steps. It is not certain, for instance, that the apparent selectivity of human observers for sinusoidal gratings of specific spatial frequency does indicate that spatial frequency analysis corresponds directly to an analysis of size. But what is certain is that, although micro-electrode experiments have allowed an enormous advance in the understanding of how neuronal mechanisms operate, their *modus operandi* in perception will only be understood by comparing these experiments with psychological results. The adaptation technique may be just what is required to pick the human visual system apart in a bloodless manner, and to reach an understanding of the detailed series of operations on which perception is based.

LIQUID HELIUM

Vortex Ring Detector?

from a Correspondent

THE experiment of Gamota and Barmatz (*Phys. Rev. Lett.*, **22**, 874; 1969) did not, as had been supposed, measure the impulse of a beam of quantized vortex rings, according to a recent paper by Huggins (*Phys. Rev. Lett.*, **29**, 1067; 1972).

At the temperature of 0.3 K at which the experiment was conducted, liquid ⁴He is almost entirely composed of superfluid, which means that macroscopic objects or excitations such as vortex rings can move through the liquid without being subject to any dissipative forces: the viscosity is essentially zero. A vortex line consists of a cylindrical core which may or may not be hollow, surrounded by rotating fluid whose tangential velocity at any point is inversely proportional to its distance from the axis; and a vortex ring consists of a vortex line whose ends have been bent round and joined up to give a toroidal shape. As each side of the ring is subject to the velocity field of the other, it is clear that the ring will have an intrinsic velocity through the surrounding fluid which depends only on its diameter and the magnitude of the circulation (which is a measure of how fast the fluid rotates about the core).

A smoke ring is a familiar classical example of a vortex ring, but rings in superfluid helium differ from their classical counterparts in that the circulation is (singly) quantized. Much of the importance attached to studies of vortex motion arises from this quantization, which implies that any element of superfluid vortex is identical to any other. Indeed, the most convincing determination of the quantum of circulation was carried out simply by studying the relationship between energy and velocity for large vortex rings. Both positive and negative ions are very strongly attracted to the cores of vortices; and an ion accelerated to a sufficient velocity in the superfluid will actually create a charged vortex ring, which is how vortex rings are usually prepared experimentally. Before the Gamota-Barmatz experiment the arrival of a beam of vortex rings was always detected by means of the minute electric current which they carried (typically 10^{-13} A).

Gamota and Barmatz set out to try to detect the arrival of a beam of charged rings not by using their charges,

but by measuring their momentum impulse, which, it was hoped, would give rise to a tiny but measurable force on a plate mounted perpendicular to the beam. They arranged for the beam of rings to impinge on a very thin flexible plastic diaphragm, metallized on one side, which formed one half of a parallel plate capacitor, and then looked for small changes in capacitance as the beam was switched on or off. Changes in capacitance, proportional as expected to the areas of individual rings forming the beam, were indeed observed, and the experiment was hailed as the first demonstration of a vortex ring detector which did not depend on the charges carried by the rings.

As the formation of uncharged rings had been postulated by several people to explain some very interesting macroscopic quantal phenomena occurring in superfluid flow near orifices, the detector seemed to have immediate application in checking their theories. But doubts soon emerged. Vortex rings in a bounded fluid can be shown to carry no momentum, so that it is far from obvious that a ring will impart

Did the Highlands move Right or Left?

THOSE with a fancy for large scale tectonics may reach for their atlases when they have seen the reinterpretation of the Great Glen fault by Garson and Plant. In next Monday's *Nature Physical Science* (November 13) they suggest that the horizontal movement of the crust along this vertical fracture carried the crust north-west of the fault to the right, rather than to the left as Kennedy proposed in his revolutionary hypothesis a quarter of a century ago. Kennedy's interpretation of the Great Glen structure as a transcurrent fault was a master stroke. Not only did it indicate the magnitude of past horizontal movements, but, as Blackett has emphasized by demonstrating the reality of such movements, it encouraged physicists, new to the field, in their search for conclusive evidence of continental displacements.

E. M. Anderson pointed out at the reading of Kennedy's paper that the fault is probably part of a more extensive structure. Time has shown him to be right, and, as Garson and Plant indicate, it forms part of a largely Palaeozoic fault system, which, before the opening of the Atlantic, extended some 3,000 km from the Maritime Provinces of Canada to the Arctic. The British section, some 1,200 kilometres long, has been traced from the continental slope north-west of Ireland through the Great Glen and the Shetlands to a point on the slope north of that group of islands. The implication of their interpretation is that a piece of continental crust the

size of England lying between the fault and the margin of the deep Atlantic has moved 100 km north-east relative to the rest of Britain.

Flinn, of Liverpool University, has done much to locate the course of the fault system north of the Scottish mainland, and it was his work in Shetland followed by his more recent analysis of the magnetic data provided by the Institute of Geological Sciences which first indicated large dextral displacements of the order of 60 km along the fault northwards for several hundred kilometres from the Moray Firth. Flinn, like Holgate, who had recognized dextral movement further to the south-west, was prepared to accept Kennedy's evidence that where the fault crossed the Scottish mainland the movement was principally sinistral. What Garson and Plant have now done is to suggest that when one considers the evidence along the length of the fault as a whole, everything combines to indicate a dextral displacement and that the time has come to abandon the original hypothesis. They go further than this and point out how such movements can be reconciled with an anti-clockwise rotation of Greenland and eastern Canada accompanied by a 3,000 km transcurrent fault system comparable in magnitude with the chief active faults of today. Before the opening of the Atlantic the sliver of European crust to the north-west of the Great Glen was, of course, joined to Greenland and Canada.

any of its fluid impulse to a boundary on which it impinges. What, then, was the force which Gamota and Barmatz measured?

This question now seems to have been answered in a typically lucid fashion by Huggins. He suggests that the force was not due to the momentum impulses of rings striking the diaphragm, but, rather, was caused by the electric field applied during their creation and subsequent motion. The fluid was in a sealed container so that its momentum, and thus the net force acting on it, must have remained zero. The force exerted on the liquid by way of the charged cores of the rings must, therefore, have been balanced by another equal but opposite force, a reaction which could only have been provided by the diaphragm and the chamber walls. It was this force, according to Huggins, which was measured.

He goes on to suggest an ingenious modification to the experiment which could enable these ideas to be verified or contradicted. If the single capacitor were to be replaced by two identical detectors side by side, which could both equally "see" the creation process and subsequent motion of rings in the electric field, then, if Huggins is correct, no change in relative capacitance would be observed as the beam is deflected from one detector to the other. It is much to be hoped that someone will carry out this test in the very near future.

METEOROLOGY

Rain in the Philippines

from a Correspondent

IN July 1972, excessive falls of rain occurred in Luzon Island of the Philippines, covering the region from Manila to Baguio. Both Manila and Baguio reported a monthly total which exceeded the mean annual totals.

In the spring of this year, a Casella Jardi type gauge, which records the instantaneous rate of fall on an autographic chart, was installed by the United

Percentage Frequency of Peak Rates of Rain-fall during Successive Intervals of 15 Min in Baguio, July 1972

Peak rate of fall in an interval of 15 min mm h ⁻¹	Frequency % (2,976 cases)
0-20	23.21
21-40	36.38
41-60	17.60
61-80	7.14
81-100	5.64
101-120	4.00
121-140	2.60
141-160	1.77
161-180	1.09
181-200	0.38
201-220	0.10
221-240	0.06
241-260	0
261-280	0
281-300	0.03

Maximum value 288 mm h⁻¹

Nations Development Programme of the World Meteorological Organization at the weather station in Baguio. This instrument has had very limited use during its period of manufacture, and few analyses of records have been made. The series of charts for July 1972 for Baguio provide a unique set of records for tropical rainfall of high intensity.

During July, 4,774.5 mm of rain fell. The previous highest monthly fall was 3,462.0 mm in August 1919. The table shows the frequency of occurrence of the

peak rates of fall during successive intervals of 15 min, as extracted from the gauge traces. A noteworthy feature is that the frequency of peak falls of 21 to 40 mm h⁻¹ is greater than those of less than 20 mm h⁻¹, which includes all periods of 15 min when no rain fell at all.

It is not possible to assess the length of time during which the peak rates actually took place. The charts suggest very sharp pulses which probably lasted from 10 to 60 s as successive cumulonimbus chimneys passed over the station.

Identification of a Southern Sky Uhuru Source

A POSSIBLE identification of the Uhuru source 2U 0328 -52 with a rich cluster of galaxies extends a pattern which has already become apparent in the northern sky. So far, five out of nineteen northern Uhuru sources with high galactic latitudes have been identified with rich clusters; in next Monday's *Nature Physical Science* (November 13) Guthrie, of the Royal Observatory, Edinburgh, points out that a similar identification is possible for 2U 0328 -52, in the southern sky. The Uhuru team at American Science and Engineering has already suggested the galaxies

IC 1954 and NGC 1249 as possible counterparts to this source, but Guthrie's contour map of the number density of galaxies in the region strongly suggests that neither identification is correct, for the centre of the error box for the source is even more close to a notably rich cluster of galaxies.

The discoveries of extragalactic X-ray sources associated with clusters of galaxies, rather than individual members, may be interpreted as supporting the idea that there is a large amount of intergalactic gas in some clusters at least.

Crustal Plates near Easter Island

By a combination of good luck and careful planning the first detailed magnetic studies of mid-oceanic ridges were of areas which revealed relatively simple patterns, namely the Reykjanes Ridge to the south of Iceland and the Juan de Fuca Ridge off Vancouver. By contrast, in the region to the north of Iceland at least three spreading ridges have existed in different places during the past 55 million years. Similarly, near the Azores, three ridges intersect to form a triple junction which, in hindsight, now appears simple by comparison with those of the Eastern Pacific near the Galapagos and Easter Island. In these areas, it was originally thought that a "simple" Azores pattern existed and that the divergence of the Pacific-Cocos - Nasca and Pacific - Nasca - Antarctic plates could be understood in terms of it. The magnetic patterns in both areas, however, turned out to be very difficult to reconcile with a simple model, and in the Easter Island region, in particular, the distribution of earthquakes (which mark actively spreading ridges) is also peculiar.

The "non-luck" part of scientific advance comes from painstaking studies of such abnormalities, and much energy and thought, particularly by Ellen Herron and her colleagues at the Lamont-Doherty Geological Observatory, have been devoted to these problematical areas. Her latest ideas on the region around Easter Island are out-

lined in next Monday's issue of *Nature Physical Science* (November 13) and involve the addition of two small plates within the three plate picture of a few months ago. Such detailed analyses can only follow very well planned field work, and the implications of such work go far beyond explaining the evolution of a remote part of the Pacific Ocean.

The rate of formation of new crust along spreading ridges can be defined from the observation of polarity events (brief periods when the geomagnetic field was in the opposite direction and during which the seafloor may only have moved 10 km or so) within the magnetic polarity sequence measured by magnetometers towed some 3 km or so above the igneous rocks of the ocean floor. Obviously the detection of brief events means that the formation of new crust must take place quite locally, yet this would seem to be contradicted by the scale of things required for the differentiation of mantle material to form new crust or by the offsets of the magnetic patterns by several hundred km along transform faults, which suggests that the mechanism is on so large a scale that such offsets are trivial. Although Herron's ideas do not yet answer these problems, they certainly impose precise limits on future considerations of any mechanisms of crust formation and they are also of considerable intrinsic interest.

Trends in Physics examined at Wiesbaden

by our Special Correspondent

This article deals with some of the topics discussed at the recent general conference of the European Physical Society.

THE second general conference of the European Physical Society was held in Wiesbaden between October 2 and 6—some three and a half years after the society's inaugural jamboree in Florence. The title of the Wiesbaden conference—Trends in Physics—implied a coverage of subjects as broad as that of the first conference but there were two innovations: a lecture by an economist on the now almost mandatory topic of the relationship between the environment, scientific research and economic policy, and a discussion centred on a paper rather quaintly called "Physics and Society—Public Policy and Current Proposals" which has been prepared during the past year or so by one of the society's advisory committees.

Pay Scientists Less?

The economist was Professor J. Tinbergen (The Hague). He talked in general terms of the need to coordinate scientific research on topics that are actually very intimately connected—for example research leading to increased human survival and research on contraception. But, as if to establish his position somewhere in the middle ground of the environmental debate, he made it clear that the backdrop against which this research is being carried out is not an entirely gloomy one. Thus for instance the population of India has increased at a rate of 2.0 per cent a year during the past ten years, he said, rather than by the 2.25 per cent a year estimated a few years ago. And in some countries in eastern Asia it turns out that a trend towards lower birth rates can be detected when family incomes exceed a much smaller limit than was previously thought. He said of the world models of Forrester and Meadows that they are "highly simplified structures", but that they are nonetheless "highly useful by virtue of their provocative character".

What should be the objectives of scientific research? Professor Tinbergen thinks peaceful change, the reduction of mass poverty and the removal of environmental problems "by shifting the frontier between knowledge and belief". But he said that the increasing need for teamwork in science has its disadvantages, for it allows manipulation by the "political-military complex". The nature of political structures inevitably leads to short-sighted planning, he said, and long term interests must be in the charge of organizations separate from those concerned with day to day affairs.

Efforts to solve the problems of pollution and exhaustion of resources require "new forms of public intervention and private initiative", according to Professor Tinbergen, one of which might be the creation of mixed private and public agencies.

Because research so often reflects the interests of the group or institution financing it, Professor Tinbergen said, it is all too often the problems of the rich countries of the world that are looked at. He did, however, hold up the Ford Foundation and the Rockefeller Foundation as examples of bodies whose support of, for example, food research has made

them exceptional cases. He also mentioned the way in which Philips has devised manufacturing processes appropriate to developing companies, in the best tradition of "intermediate technology".

Professor Tinbergen also spoke of the two types of capital—physical and human—the latter of which comprises the innate capabilities of human beings. Human capital, he said, is able to "exploit" in much the same way as physical capital, and the natural effect of the process of education is to increase human capital, that is to produce "human productive qualities". On more than one occasion during his lecture Professor Tinbergen spoke of the virtues of distributing a country's income more equally, so it was perhaps not surprising that he should end with the prediction that "no scientist need be unemployed, though his salary may need to be adjusted downwards".

Much of the preamble in the paper on physics and society is a potpourri of things that have been said many times before in many places. Most people are by now aware that the general public at times views scientists with some suspicion as vain and blinkered people who prefer an isolationist existence. But what to do about it? Clearly either that supposition has to be shown to be incorrect or something has to be done to rectify the state of affairs if it does in fact exist.

The paper sets out the belief that "it is now necessary to select specific public apprehensions and to arrange for correspondingly specific comprehensive articles to be prepared in reply, by lecture or publication in appropriate fora". It goes on to say that the European Physical Society should "stimulate the dissemination of scientific news by authoritative sources, from which mass circulation newspapers could freely quote—for instance in the form of a rapidly appearing, easily printable and readable, scientific newspaper".

The discussion provoked by the paper was disappointingly unspecific. Most of the debate was no more than an attempt on the part of the participants to find out whether a problem actually exists. In the final analysis there seemed to be two points of view—that physicists should make a real effort to reveal themselves and their work and that this is altogether too defensive an attitude. The feeling expressed by some participants was that the European Physical Society should not be seen to denigrate the profession it exists to sponsor. Professor A. Abragam (University of Paris) advocated a professional public relations exercise to "sell" physics and to drum up more public support for research.

Professor E. H. S. Burhop (University of London) took the view that physics has a poor image because there is a lack of social responsibility among physicists, and his contribution to the debate led others to suggest that the EPS should stimulate discussions "on all problems concerning the social responsibility of physicists" and that specific problems arising from time to time should be considered on a time scale of weeks by committees of the society. Two motions presented to the general assembly of the European Physical Society embodied some of the ideas put forward during the discussion, and also called for the EPS, possibly through a new committee, "to pay special and continuing attention... to matters of policy, planning and priority in physics research, development and education with particular reference to the supply of and demand for manpower". (Although the motions could not be voted on because insufficient notice was given of the inten-

tion to propose them, Professor H. B. G. Casimir, president of the EPS, said that their contents would be carefully considered by the society's executive committee.)

The rather blurred outcome of the open discussion contrasted markedly with the clear and cool appraisal of the relationship between physics and society by Professor Casimir in his plenary lecture. "Since we are unwilling and probably unable to stop the development of science and technology and since we are quite obviously unable to design a comprehensive masterplan for science and society, our best chances for gaining some control over the ominous spiral lie in a plurality of controls, in an independence of opinion of the several participating groups, and in openness". In particular he said that universities should maintain their independence of industry and industry should respect this independence.

Universal Origin to Gravitational Radiation

One of the plenary lectures and several of the addresses in the parallel sessions—which were organized by the nine subject divisions of the EPS—dealt with various aspects of astronomy, a subject which seems to be capturing physicists' attention increasingly these days. Professor W. Kundt (Hamburg) outlined the problems that still prevent an understanding of the origin of the universe. The principles are laid, he said, but the details are missing. In particular the existence or not of a superbaryon, with a baryon number greater than unity, has a great influence on models of the development of the hadron era (between 10^{-23} s after the big bang and 10^{-4} s or 1,000 yr later depending on the model). Professor Kundt revealed that some theoreticians are now even focusing their attention on the period before 10^{-44} s, when the diameter of the light cone of the universe was much smaller than the de Broglie wavelength of an elementary particle; this can only be understood as a quantum phenomenon, Professor Kundt said.

During the hadron era, which ended when all the hadrons had decayed, galaxies may well have started to form as the result of thermal fluctuations. Then followed the radiation era, lasting until 10^5 yr and possibly seeing the conversion of H to He and the creation of seed magnetic fields, and finally the present matter era in which the fine structure of the universe—comprising galaxies, stars, quasars and so on—was established. As for the average mass density of the universe, Professor Kundt said that it still remains to be found out whether it exceeds the critical 5×10^{-30} g cm $^{-3}$ necessary for expansion and contraction to be feasible (rather than expansion for ever).

One of the parallel sessions was a joint effort by the physics in astronomy division and the computational physics division. Dr F. Hohl (NASA, Hampton) described, with the aid of a fascinating film, his work on the simulation of the development of a rotating disk of stars with a computer. His model uses a two-dimensional array of cells (256×256) and at the centre of each cell a single mass density is defined by the number of stars in that particular cell. The gravitational field at the centre of each cell can then be computed, and hence the force acting on stars. Dr Hohl's calculations involved disks containing between 50,000 and 200,000 stars, and the use of Newton's laws of motion allowed the computer to update the velocity and position of every star after each of 200 time steps that go to make up a model of "galactic rotation". In the real situation, of course, a single rotation of a galaxy such as our own takes several hundred million years.

Starting with a uniformly rotating disk containing stars whose velocity dispersion is set by a theoretical condition for local stability, Hohl described how, during the past few years, he has progressively refined his model—for example, by allowing for cooling effects and so on—to the stage where it can generate quite respectable looking galaxies with spiral structures (Fig. 1).

In another parallel session, devoted solely to physics in

astronomy, Dr D. W. Sciama (Oxford) predicted that in six months time "we will know whether Weber was right or not" in his claim to have detected gravitational radiation. The basic findings that other experiments will have to support or refute, said Dr Sciama, are that there is one coincidence a day from Weber's two large cylinders (1,000 km apart and resonating at 1,660 Hz) and that the coincidence rate seems to be a maximum at twelve hourly intervals, when the gravitational "aerial" is pointing at the galactic centre either directly or through the "transparent" Earth.

What could be the source of the enormous amounts of energy implicit in Weber's observations? Dr Sciama pointed out that if gravitational radiation is being emitted isotropically from the centre of the galaxy, then the catastrophic events occurring there must be radiating about 10^5 solar masses a year—that is one mass of the galaxy every 1 million yr. Yet the age of the galaxy is more like 10^8 yr. So has the phenomenon detected by Weber just started? Is the bandwidth very narrow, and just that of Weber's apparatus? Is the emission from the galactic centre anisotropic? Or can all be explained in terms of a system of black holes, or a naked singularity at the galactic centre?

Even if Weber is wrong, Dr Sciama concluded, he has stimulated the technological advances necessary for the detection of gravitational waves from, say, supernovae, and has laid the foundations of gravitational wave astronomy.

Partons Prominent

It fell to Professor L. Van Hove (CERN) to guide one of the plenary sessions through the intricacies of recent developments in the physics of elementary particles at high energies. This he did with considerable skill by selecting five topics that represent the essence of what has been going on in the past two or three years. At the rather esoteric, yet important, level he outlined the way in which renormalization theory, which has been used with success for many years in electromagnetic quantum field theory, can now be used in field theories involving massive vector bosons like the celebrated intermediate boson which has been implicated in the weak

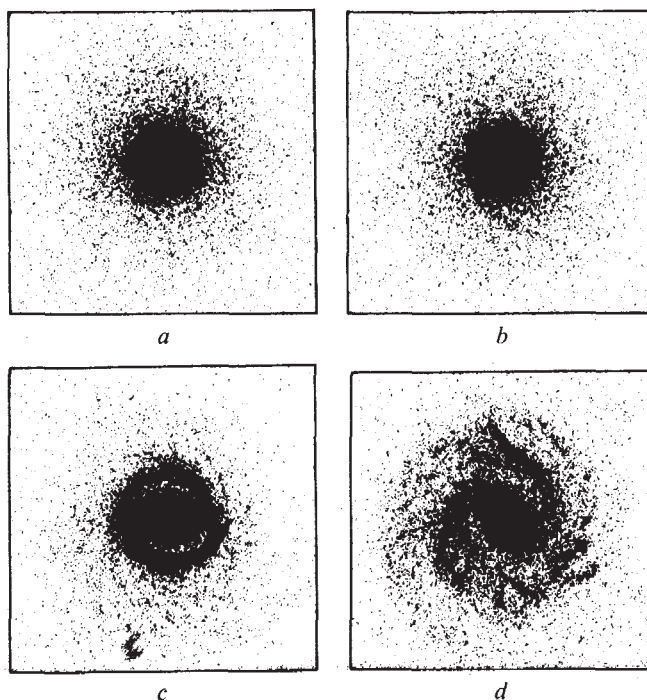


Fig. 1 Development of a rotating and cooling disk of stars¹ after a, 0 revolutions; b, 2 revolutions; c, 4 revolutions; d, 8 revolutions. From *Astrophys. J.*, **168**, 343 (1971).

interaction. This development, pioneered by 't Hooft, opens up the way to a unified description of weak and electromagnetic interactions, Professor Van Hove said.

After discussing the series of experiments that led to the important discovery that the weak interactions violate time reversal invariance, Professor Van Hove went on to talk about one of the topics that must be uppermost in the minds of most high energy physicists—the granular structure of the proton. The key experiments in this case are those concerned with the deep inelastic scattering of electrons by protons, and, not surprisingly, the work done at the Stanford Linear Accelerator Center (SLAC) is without peer in this context. The team at SLAC has found that when allowance is made for electromagnetic interactions, the cross-section for the nuclear interactions of the electron's virtual photons (that is those which produce strongly interacting secondaries like pions) depends only on the ratio M/q , where M is the effective mass of the system of hadrons generated and q is the four-momentum transferred. If the proton were simply a point charge, the cross-section would depend on M and q separately, but the occurrence of this "scaling" effect strongly suggests that the proton's charge is distributed in the form of point-like grains—Feynman's "partons". The nature of these grains, Professor Van Hove said, has become one of the central issues of high energy physics.

Professor Van Hove also revealed a surprising result which has come out of an experiment on the interactions of pions with nuclei carried out at CERN by a team made up of CERN personnel and a group from ETH Zurich. The CERN-ETH experiment showed that a group of pions produced when a single pion interacts with a nucleon on its way through a nucleus will then itself be absorbed no more strongly than a single pion as it goes on through the nucleus. The explanation seems to lie in the fact that constituents of the multipion system are mostly in virtual states.

Some of the topics alluded to by Professor Van Hove were also taken up in the parallel sessions. In particular Dr A. Wetherell (CERN) and Professor A. Zichichi (Bologna) talked about the physics now being done with colliding proton beams (at the CERN intersecting storage rings) and with colliding electron beams (at Frascati for example). Dr Wetherell said that the experiments at the ISR, which are equivalent to those that could be done with conventional accelerators producing protons of energy between about 300 GeV and 2,000 GeV, are basically trying to answer five questions. What secondaries are produced in proton-proton collisions and how are they correlated? Are there hadronic states with very high masses? Do quarks exist or are they a figment of the imagination? What are partons? And, finally, is there such a thing as the vector boson—the elusive mediator of the weak interaction?

Professor Zichichi, who must surely share with Professor N. Cabibbo the prize for squeezing as much physics as possible into the allotted time, summed up the state of colliding electron beam physics quite succinctly by saying that quantum electrodynamics is now known to be "good" up to squared four-momentum transfers of 9 (GeV/c)^2 , that no heavy lepton has been found to an upper mass limit of 900 MeV, and that the ρ' isovector meson is now well known to have a mass of 1.6 GeV and a width of 900 MeV.

Lasers Come into Their Own

In his lecture on non-linear optics Professor W. Kaiser (Munich) neatly summed up the tremendous advances that have been made in the techniques for producing short and powerful light pulses during the past decade with one slide, the essence of which is shown in Table 1. Having discussed in an elegant and straightforward fashion the state of the art as far as non-linear processes are concerned, including the optical parametric amplifier which can be tuned by changing the temperature of the crystal, he went on to mention the

spin-flip Raman laser—a device also close to the heart of a lecturer in one of the parallel sessions, Professor S. D. Smith (Heriot-Watt University). This kind of laser can be tuned between about 10 μm and 15 μm , and finds application as an extremely good spectrometer, capable for example of detecting 1 p.p.m. of NO with ease. (The noise level can be equivalent to as much as a hundred times less than that.) The spectroscopist now sees the tunable light source as a development to be welcomed, said Professor Kaiser.

Table 1 Recent Achievements with Lasers

Year	Minimum pulse length s	Peak power W
1960	10^{-6}	10^3
1962	10^{-8}	10^6
1966	10^{-11}	10^9
1972	5×10^{-13}	$10^{10}-10^{12}$

Professor Kaiser's lecture was complemented by several others, including one by Dr G. Fiocchi (Frascati) whose chosen topic was the use of lasers in the atmospheric sciences. He began by describing the way in which a laser and a receiver can be used to measure the concentration of aerosols—particles with diameters between about 0.01 μm and 20 μm —in a 15 m cube at a height of about 70 km. Lasers can also be used to probe other atmospheric constituents, Dr Fiocchi said, including sodium, calcium, potassium and lithium, much of which probably originates in meteoroids. He also testified to the wide applicability of the spin-flip Raman laser by saying that it would have an important part to play in the monitoring of pollutants like SO_2 , NO_2 and H_2S at heights up to about 1 km.

No conference just now would have been complete without a discussion of the prospects for nuclear fusion induced by lasers. Standing in for Professor L. Artsimovich (Moscow), who, like several other Russian physicists, was unable to attend the Wiesbaden conference, Professor A. Schluter (Max Planck Institute, Garching) spoke about the problems of fusion, and said that he thinks that the arrival of lasers on the scene should not be heralded out of hand as the "answer", but rather that the same attitude should be adopted to them as was adopted to magnetic confinement methods twenty years ago—namely that the idea is "fascinating". Professor Schluter considers the toroidal devices like Tokamaks still to have the edge over all comers. He said that the cost of a prototype reactor is not likely to be less than \$1,000 million.

Physics and Display

Central to the lecture by Dr S. van Houten (Eindhoven) on the physical aspects of displays was the fascinating phenomenon of the liquid crystal—an organic substance whose molecular arrangement shows some degree of long range order and whose characteristics are somewhere between those of a solid and a liquid. In the simplest of display devices based on liquid crystals, a nematic crystal—one with all its dipoles pointing in more or less the same direction—can be made to scatter light in well defined regions by the application of a small electric field. If the crystal is sandwiched between two pieces of glass about 20 μm apart, and if suitably shaped transparent electrodes are placed on the inside surfaces of the cell, then about 20 to 40 V can cause local turbulence as a consequence of the anisotropy of the electrical conductivity. The current and field are not parallel so a space charge is created, causing a shear flow, more space charge, and eventually turbulent flow and scattering of any incident light.

Many more subtle approaches are possible—for example using cells containing a crystal with some molecules perpendicular to the cell faces and some parallel to them. Overall, display devices of this kind have the great advantage that they are compact and consume relatively little power.

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Magnetic Smooth Zones in the South Atlantic

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Aeromagnetic profiles show two types of smooth magnetic zone in the South Atlantic symmetrically positioned about the Mid-Atlantic Ridge. The first borders the continental margins off Argentina and the Union of South Africa and the second extends over part of the Argentine-Brazil and Angola-Cape basins. The origin of the first zone may be related to the tectonic and/or diagenetic processes associated with the initial rifting of South America from Africa and the second can be simply explained by seafloor spreading during Middle-Late Cretaceous time when the polarity of the Earth's field was relatively constant.

MAGNETIC anomalies over the ocean basins are generally arranged linearly parallel to the mid-oceanic ridges and have amplitudes on the order of a few hundred gammas. But in some regions, notably along the continental margins, anomalies are nearly non-existent. In the North Atlantic a continuous smooth zone is believed to extend from Newfoundland to the Bahamas in the western basin, and from Ireland to the Cape Verdes in the eastern basin¹⁻³.

Few magnetic studies in the South Atlantic have been published to date. Dickson *et al.*⁴ have correlated and dated several of the magnetic anomalies; their results have been confirmed by the JOIDES drilling results in this area⁵. But smooth zones were not explicitly discussed. More recently, Van Andel and Moore⁶ presented a profile from Brazil to Angola, which shows a magnetic smooth zone about 500 km wide over the Angola abyssal plain and a magnetic profile has been obtained in the same area by RV Chain (unpublished data). Simpson and DuPlessis⁷ have published several magnetic profiles from the margin of south-western Africa, and Ladd and others (unpublished) noted the existence of so-called "quiet zones" south and south-east of the Rio Grande Rise to the west of anomaly 31, as well as east of anomaly 31 south-east of the Walvis Ridge, but did not map them.

We have examined aeromagnetic profiles from Project Magnet⁸, which have become available since the publication of the earlier work, to obtain a more detailed account of the magnetic pattern in the south Atlantic and particularly of magnetic smooth zones (Fig. 1). We constructed the profiles using observed total intensity field points digitized at a 2 to 9

km interval along the track depending on the spatial wavelength of the total field variation. The regional geomagnetic field variation was removed using the International Geomagnetic Reference Field Epoch 1965. Anomaly profiles are plotted with their amplitude normal to the vehicle track (Figs. 1, 2 and 3). The altitudes ranged from 1,000 to 3,000 m. But we are confident of the anomaly comparisons for two reasons: first, profiles from the same area give comparable magnetic pattern (Argentine Basin); second, good correspondence exists between our profiles and shipboard data (Fig. 4) when available.

South American Side

Several profiles (Figs. 1, 2) off the Argentina continental margin indicate the existence of a smooth zone. Some Vema 16 data (J. R. Heirtzler, unpublished) confirm the existence of such a zone, but the small number of profiles in this area do not allow precise mapping of this zone. It appears to be parallel to the 2,000 fm isobath. Its seaward termination is frequently marked by a large anomaly. The average width is on the order of 300 km.

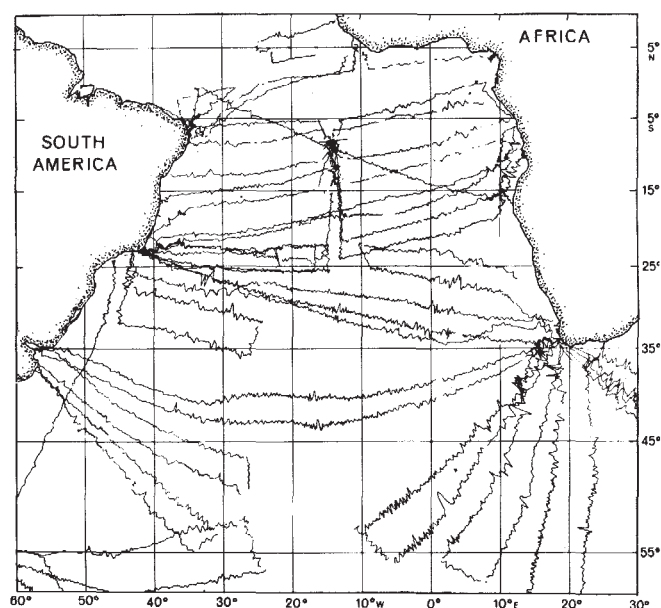


Fig. 1 Flight tracks from the Project Magnet along which total intensity magnetic data were processed for this study. Positive anomalies are plotted on the northerly side of the track except for those tracks whose azimuths are within 20° of north. For these the positive anomalies are plotted on the westerly side of the track.

Further seaward off Argentina and Brazil, the most notable feature is a second smooth zone (Fig. 2). Although somewhat less smooth than the first one, it is distinct from the adjacent magnetic anomaly pattern. Over the Brazilian Basin between 15° S and 25° S latitude, 10 profiles show this zone with an average width of 350 km. At about 30° S, over the Rio Grande Rise, the magnetic pattern is completely different. South of the Rio Grande Rise six profiles identify another portion of the smooth zone over the Argentina Basin where the zone is 400 km wide. The zone seems to be interrupted only by the presence of the Rio Grande Rise.

African Side

Fig. 3 presents a pattern similar to that which we have described off South America. Profiles south and west of Cape Town show a magnetic smooth zone along the continental margin. Several large anomalies interrupt it west of Cape Town. Towards the east the Agulhas Bank possesses

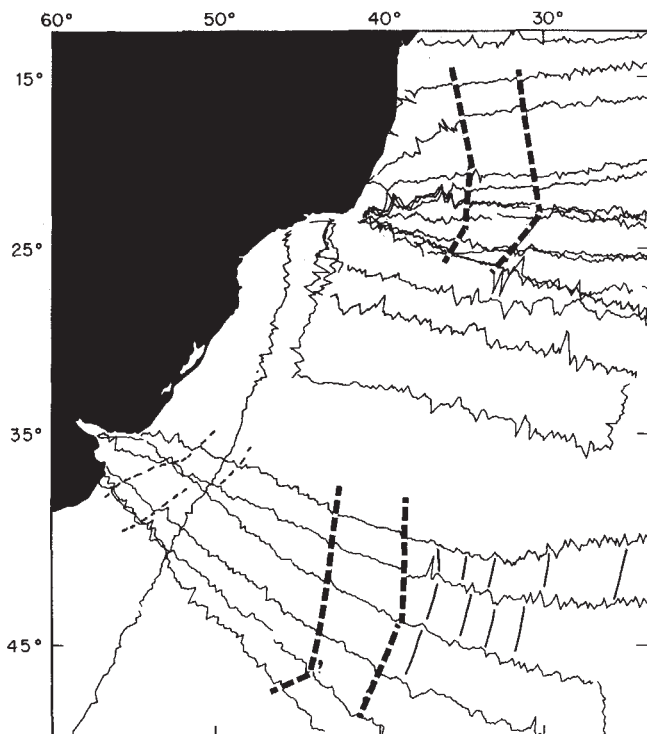


Fig. 2 Magnetic anomalies on the South American side.

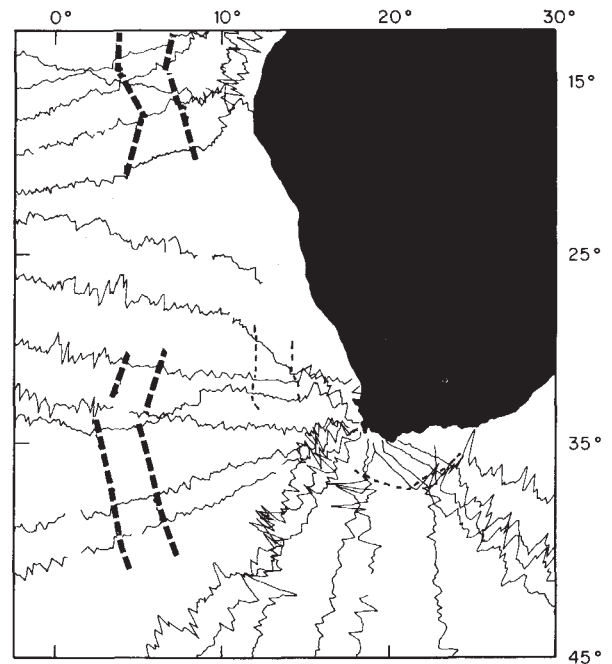


Fig. 3 Magnetic anomalies on the South African side.

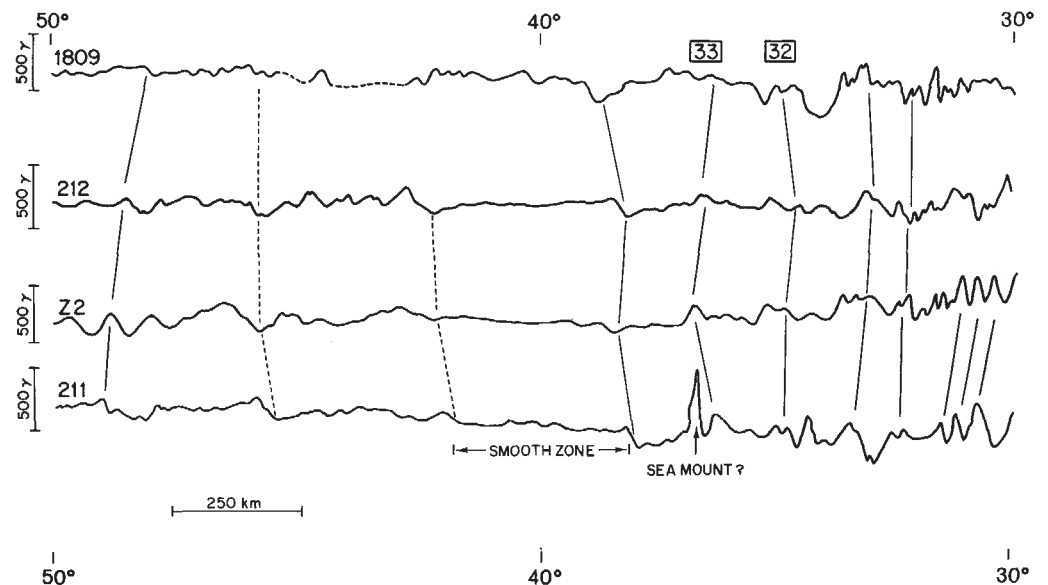
a complex anomaly pattern which also abruptly terminates the smooth zone.

Seaward, in the Cape Basin, between latitudes 30° S and 40° S, a second relatively smooth magnetic zone similar to the zone previously described over the Brazil and Argentine Basins appears. Other profiles also suggest the presence of a smooth zone here. The zone is abruptly terminated by the Walvis Ridge. North of the Walvis Ridge in the Angola Basin, a smooth zone is visible on several of our profiles, and other⁶ profiles further delineate a smooth zone here. This zone is probably in continuity with the one observed over the Cape Basin.

Discussion

The locations of the magnetic smooth zones are shown in Fig. 5. We distinguish two different zones: the first is a discontinuous zone found along the continental margins in a position similar to that described by others in the North Atlantic¹ and along the north-western African margin^{2,3,9-11} which we term marginal smooth zones; the second type, characterized by a less smooth pattern, lies in the deeper

Fig. 4 Comparison of project magnetic anomaly profiles (211, 212) over the Argentina Basin with shipboard profiles (z, 1809) (see Fig. 1). The anomaly numbers are those given by Ladd *et al.* and based on shipboard profiles extending between 30° S and 50° S (see Fig. 5).



part of the South Atlantic principally over the abyssal plains, and we term these abyssal smooth zones.

Both zones seem to be symmetrically arranged with respect to the Mid-Atlantic Ridge (Figs. 5 and 6).

Magnetic smooth zones characterize the continental boundaries of North Atlantic^{1-3,12}, but its geological interpretation is still uncertain^{1,2,11,13,14}.

In any event, an age of 190–155 m.y. suggests a spreading rate of about 1.2 cm yr^{-1} which corresponds well with observed spreading rate before anomaly 31 and the initial opening of the western North Atlantic^{2,12}. Different ages given by the JOIDES drilling results in the smooth zone on both sides of the North Atlantic¹⁰ do not permit a simple explanation for formation of this zone, and possibilities such as demagnetization¹⁵ or volcanic intrusion into a thick sedimentary deposit¹⁶ may be still a part of the explanation.

Marginal Smooth Zone

The available data are insufficient for a detailed analysis of these smooth zones, such as has been possible in the North Atlantic¹⁻³, but several important features can be noted: the position of the zone, its width and its magnetic pattern

when distinguishable, are comparable to those in the North Atlantic; this zone appears to be developed along broad continental margins, off Argentina and the Republic of South Africa, and where the margin is narrow the smooth zone is poorly developed or non-existent.

Difficulties arise when one attempts to use the various explanations proposed for the North Atlantic smooth zone, notably the fact that opening of the South Atlantic began 135–140 m.y. BP (refs. 4, 17, 18). It does not seem possible that ages of marginal zones in the North and South Atlantic could be the same; ages attributed to the North Atlantic smooth zone range from the Late Palaeozoic to Lower Jurassic^{1,2,14}. There is a possible relation between smooth zones and evaporite layers marked by diapirs in the North Atlantic¹²; we find this hypothesis difficult to accept. For example, off Angola, Gabon and Brazil, there is abundant evidence of diapiric structures of probable evaporitic composition even though marginal smooth zones do not seem to be present^{19,20}.

Because these marginal smooth zones are not found throughout the length of the South Atlantic, we find it difficult to attribute their origin simply to the rotation of continental plates. The fact that they are best developed along broad continental margins suggests their origin may be related to

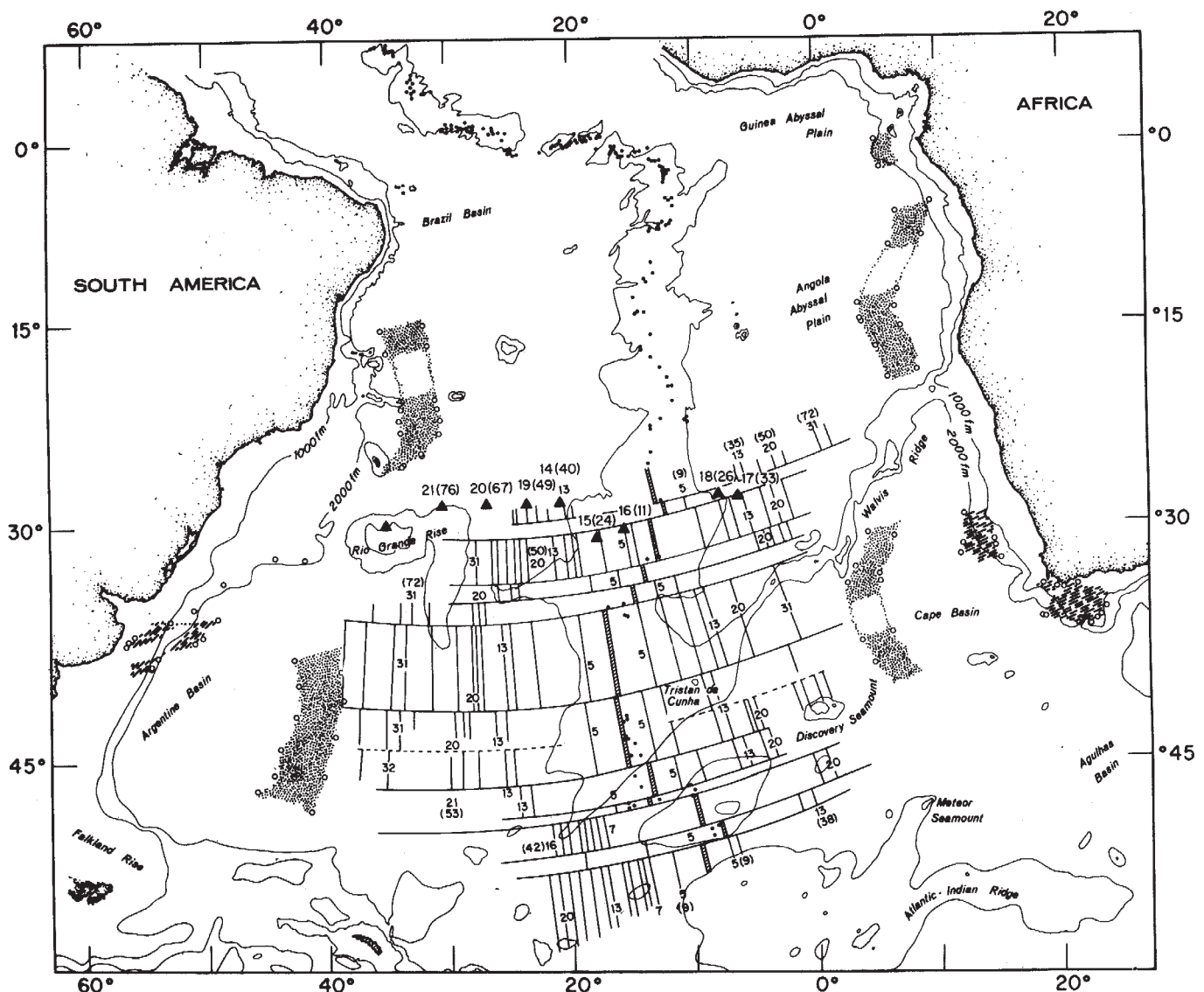


Fig. 5 Chart showing locations of the South Atlantic smooth zones. Stippled areas represent abyssal smooth zones. Hatched areas indicate marginal zones. Open circles mark the boundaries of smooth zone on each profile. Numbered magnetic anomaly identifications, ridge crest position and fracture zone pattern are from Ladd *et al.* The fractures are aligned along small circles drawn about the South American-African rotation pole of Bullard *et al.* Solid triangles show the position of the DSDP results, ages are given in parentheses. Earthquake epicentre locations are from Barazangi and Dormann.

diagenetic processes such as chemical metamorphism¹⁵ or to the demagnetization of the magnetic layer² beneath a thick sedimentary cover. A correlation of deep filled basins with smooth magnetic profile is shown by a profile across the Agulhas Bank²¹. The correspondence of a marginal magnetic smooth zone off Argentina with an important deep basin (Rio Salado Basin)²² confirms this idea.

Further initial opening of the South Atlantic is dated at between 135 and 140 m.y. BP (refs. 17, 18). This period, according to the time scale reconstruction^{14,23} and to the seafloor spreading interpretation²⁴, is characterized in the North Atlantic by numerous reversals which lasted until 110 m.y. BP. It is doubtful that a smooth zone would be created during this period (Serra Geral Zone or Keathley sequence). Accordingly we favour a diagenetic origin for the marginal smooth zones resulting from burial beneath a thick sedimentary cover.

Abyssal Smooth Zones

The distribution of these zones seems to coincide with the deepest areas of the South Atlantic, that is the southern Brazil Basin, Argentine Basin, Angola Basin, and the Cape Basin.

The north-south continuities of these zones on both sides of the ridge are interrupted and offset in a right-lateral sense by the Rio Grande Rise on the west and the Walvis Ridge on the east. The oceanic magnetic anomaly pattern between 45° and 30° S across the ridge flanks is also offset by these features in a similar manner (Figs. 2 and 3).

The positions of these zones are nearly symmetrical with respect to the Mid-Atlantic Ridge axis and the anomaly pattern identified up to 72 m.y. BP (anomaly 31).

South of the Rio Grande Rise and Walvis Ridge the smooth zones appear to be asymmetrically arranged. That is, the smooth zone width off Argentina as measured along small circles drawn about the South America-Africa rotation poles of Bullard *et al.*²⁵ is about 15% greater than the zone off Cape Town. A similar relationship for the distance away from the ridge crest and for the anomalies (for the past 80 m.y.) can be observed (Fig. 6).

If the zones are formed by seafloor spreading, they should be explicable in terms of present knowledge of the plate rotation geometry for Africa and South America. For example, several authors^{18,26}, using marginal offsets and the fracture zone directions, have proposed an evolution of the South Atlantic from the initial rifting from 135 m.y. to about 80 m.y. If we assume the borders of abyssal smooth zones to be isochronous, and if the calculated poles of rotation prevailed during their formation, we expect a reasonable fit of the zone boundaries. Fig. 6 shows that a good correspondence exists. But a partial super-position and minor divergence of the boundaries suggest that the early opening pole may not be applicable for the entire 135–80 m.y. period or that the rate of opening may be somewhat fast.

We can also argue that the good correlation between the basins and these abyssal smooth zones is a result of demagnetization of the oceanic basement as we implied above for the marginal zone. But the geometric distribution of these zones with respect to the ridge axis suggests that they are more probably related to seafloor spreading during a long period of constant geomagnetic polarity.

It has been shown²⁷ that during the Cretaceous there was a long period of normal polarity related to an observed smooth zone in the Pacific²⁸ occurring beyond anomaly 32 (80 m.y.). A new time scale shows a long period of constant polarity occurring between 80 and 110 m.y. called the "Mercanton Interval". We feel confident, like others²⁹, in attributing the existence of these abyssal smooth zones to the same period of constant polarity, because this zone appears shortly after anomaly 31 (middle Upper Cretaceous). But the width and relative position of the zones suggest some modification of the time scale.

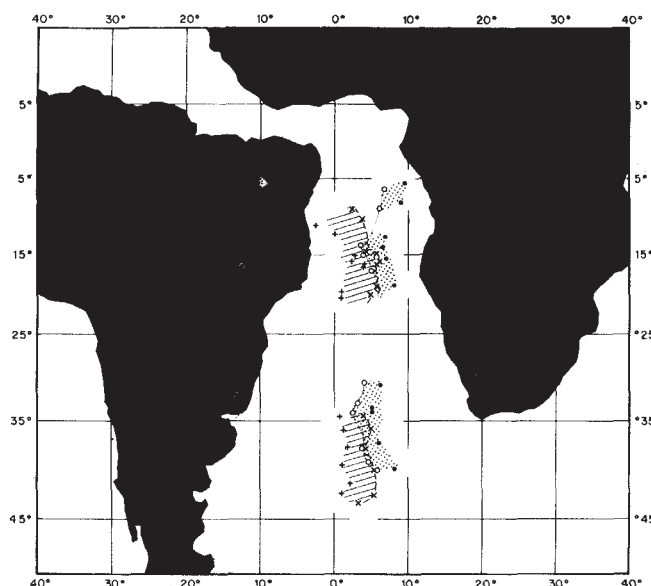


Fig. 6 Reconstruction of the South Atlantic using the rotation given by Francheteau and LePichon. South America and its abyssal smooth zone have been rotated 35.26 degrees anticlockwise in order to fit both smooth zones in the Upper Cretaceous.

Off Brazil, the average width of the abyssal smooth zone is 350 km; off Argentina it is about 400 km because it is farther from the pole of rotation. Assuming a relative constant velocity of seafloor spreading at 2 cm yr⁻¹ (ref. 2) we find that this smooth zone would represent an interval duration of between 18 and 20 m.y. which is only a part of the Mercanton Interval¹⁴ (30 m.y.). Two possibilities exist to explain this disparity: first, seafloor spreading may have been slower before Cretaceous time (about 1 cm yr⁻¹); second, the time scale¹⁴ may not include periods of more frequent reversal as suggested for the North Atlantic.²⁴

It is unlikely that a slower velocity of seafloor spreading prevailed during the Mesozoic. Many data^{12,17,18,30,31} indicate a time of opening for the South Atlantic of early Cretaceous (135 m.y.). This requires an average spreading rate of 2 cm yr⁻¹ at latitude 30° south before anomaly 31. The possibility remains that variations in the rate of opening did occur within this period.

It is not clear why this type of smooth zone has not been mapped in other areas of the world where it is known from the deep sea drilling that spreading has been active. Only the magnetic smooth zone in the North Pacific^{27,31} and the magnetic smooth zone west of Spain in the Atlantic¹² have been associated with a Cretaceous period of constant magnetic polarity. No attempt has been made to relate them directly to an absolute geomagnetic time scale. The distance from anomaly 31 to the seaward boundary of the abyssal smooth zone in the South Atlantic is about 400 km (Fig. 5). If the rate of seafloor spreading has been constant at 2 cm yr⁻¹, the seaward boundary of the smooth zone would have an age of approximately 90 m.y. BP. The age of the landward boundary would be 110 m.y. Analyses of magnetic profiles from the western North Atlantic suggest the presence of a long normal polarity period between 80 and 90 m.y. BP (ref. 24). Once again this period does not seem to correspond to that observed in the South Atlantic. It may be possible²⁴ that the Lower Cretaceous anomalies were erased by metamorphism associated with the anomalies in the basins of the South Atlantic, broadening the apparent smooth zones. But this explanation would require an even greater discrepancy with the duration and age of the Cretaceous constant polarity interval observed from land rocks. As an alternative we suggest the true width of North Atlantic Cretaceous smooth zone is not known accurately. A delineation of probable abyssal smooth zones here may be more difficult due to numer-

ous fracture zones crossing the North Atlantic basin. These fractures cause non-linear topographic "noise" type anomalies which mask what might normally be a smooth magnetic zone. In any event, an examination of the magnetic anomaly map of Pitman and Talwani¹² shows that beyond anomaly 31 a relatively smooth magnetic pattern exists. But this is somewhat rougher than that observed near to the continental margins.

We thank X. Le Pichon, K. Poehls and J. Ladd for discussions; B. Bornhold for help, and H. Stockard for providing the data. Drs K. O. Emery, J. R. Heirtzler, B. Luyendyk, and R. P. Von Herzen made many suggestions.

This work was supported by the International Decade of Ocean Exploration (National Science Foundation) and by the Office of Naval Research.

Received May 15; revised October 4, 1972.

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Insignificance of Evidence for Differences in Heritability of IQ between Races and Social Classes

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Evidence previously analysed is insufficient to support the conclusions drawn.

DURING the last few years, Jinks, Fulker and Eaves^{1,6} have systematically reanalysed many of the available data on IQ and from a combination of this experience, biometrical model-building and computer simulations we have defined both the qualitative and quantitative minimal requirements for such data if they are to yield estimates of heritability and of the genetical, environmental and interactive components of variation. We have also described kinds of data and laid down guidelines for the future collection of data that would be adequate to answer

the kinds of question that have been posed but so far inadequately answered.

Dr Scarr-Salapatek⁷ has attempted to go beyond what we have shown to be possible with the minimal set of data we considered, doing so on the basis of analyses of data which fall short of this minimal set in both quality and quantity. It is necessary, therefore, to examine the consequences of doing so.

Qualitative Inadequacies

Qualitatively, the minimal set of data considered by Jinks and Fulker¹ consists of a number of pairs of monozygotic (MZ) and dizygotic (DZ) twins, the individuals in each pair having been raised together. Such data provide an estimate of the ratio of genetical variation within families (pairs) to the total variation arising from all sources within families¹—the *H* statistic of Holzinger. This statistic is not a heritability estimate

in any meaningful sense as it omits all information about the genetical, environmental and interactive sources of variation that arise between different families (pairs). It is an estimate of broad heritability only where the ratio of genetical to all sources of variation is the same both within and between families. In addition, the minimal set of data also provides test for the presence of interactions and correlations of the genotype with the within family environment and interactions of the environmental components of variation within the between families. Such data, however, will not provide estimates of the four basic components of the total variation, namely, the genetical and environmental variation within and between families, that is, the G_1 , E_1 and G_2 , E_2 of Jinks and Fulker which are directly relatable to the σ_{wp}^2 , σ_{we}^2 and σ_{bg}^2 and σ_{be}^2 of Scarr-Salapatek.

The data presented by Dr Scarr-Salapatek fall short of this minimal set in that there is no complete classification of twin pairs into monozygotic and dizygotic. They are classified into twins of unlike sex that must be dizygotic in origin and twins of like sex that may be either monozygotic or dizygotic. With a notional partitioning of the twins of like sex into proportions that are monozygotic and dizygotic in origin, of the kind used by Scarr-Salapatek, the data become equivalent to the minimal set in one respect but fall short in all others. They provide an estimate of Holzinger's H statistic, but with a larger standard error, and no test for genotype-environmental interactions or correlations. In relating Scarr-Salapatek's derivation of the H statistic (her "restricted heritability", h_r^2) to that of Jinks and Fulker¹ and Eaves⁵ it should be noted that the σ^2 s of Scarr-Salapatek are not the variance components of the conventional analysis of variance but are the mean squares of the latter.

From the estimate of the H statistic and the corresponding total variance Scarr-Salapatek proceeds to estimate the genetical and environmental components of the variances within and between family. With only the equivalent of the minimal set of data this procedure is not possible without making assumptions¹. The nature of these assumptions can be seen from the simplest of all models (which assumes random mating and no genotype-environmental interactions or correlations) in which G_1 , E_1 and G_2 , E_2 represent the genetical and environmental components of variation within and between families as follows (see Scarr-Salapatek, Table 10):

Component	Within family	Between family	Row total
Genetical	G_1	G_2	$G_1 + G_2$
Environmental	E_1	E_2	$E_1 + E_2$
Column total	$G_1 + E_1$	$G_2 + E_2$	$G_1 + G_2 + E_1 + E_2 = \text{total variance } (V_T)$

Because

$$\frac{\text{Row 1 total}}{\text{Total variance}} = \frac{G_1 + G_2}{G_1 + G_2 + E_1 + E_2}$$

is the true broad heritability, h_b^2 , and

$$\frac{\text{Column 2 total}}{\text{Total variance}} = \frac{G_2 + E_2}{G_1 + G_2 + E_1 + E_2}$$

is the intraclass correlation, r_{dz} , for dizygotic twins, the row totals equal $h_b^2 V_T$ and $(1 - h_b^2) V_T$ and the column totals $(1 - r_{dz}) V_T$ and $r_{dz} V_T$, respectively. From Scarr-Salapatek's data we can estimate only $h_r^2 = G_1 / (G_1 + E_1)$ and to equate this statistic to h_b^2 we must assume that $G_1 / G_2 = E_1 / E_2$. This is also a necessary assumption for the next step in Scarr-Salapatek's analysis which is the estimation of G_1 , G_2 , E_1 and E_2 from the row and column totals.

The relative magnitudes of G_1 and G_2 depend on the kinds of gene action underlying the variation and the mating structure of the population¹. In the absence of both dominance and assortative mating $G_1 = G_2$, with dominance alone $G_1 > G_2$

and with assortive mating alone $G_1 < G_2$. Both dominance and assortative mating are known to occur for IQ^{1,6,8,9} and since they affect the relative magnitudes of G_1 and G_2 in opposite directions we neither expect nor find large differences between them.

The relative magnitudes of E_1 and E_2 cannot be predicted from any *a priori* model; they can only be established empirically by observation. The minimal set of data which allows the estimation of E_1 , if we assume the present model, cannot provide a direct estimate of E_2 . Thus, the assumption that $G_1 / G_2 = E_1 / E_2$, that underlies the analyses and interpretations of Scarr-Salapatek, is neither testable from the data she provides nor can it be justified on theoretical grounds. These arguments are, of course, made more complex if we attempt, as does Dr Scarr-Salapatek, to correct h_r^2 and the components of the total variation for the effects of assortative mating (her h_a^2) but this extension does not invalidate the principle we have sought to illustrate by reference to the simpler situation, namely, that her analysis involves untestable assumptions about the relative magnitudes of the genetical and environmental components.

Quantitative Inadequacies

Having commented upon the limitations imposed on the analysis and interpretation arising from the qualitative aspects of the data, we can now turn our attention to the limitations that arise from the quantitative aspects which depend on the number of twin pairs that fall within each of the racial, sex and socio-economic sub-groups. While it is the qualitative properties of the data that determine the kinds of analyses and conclusions that can be validly applied, it is the quantitative properties that determine the standard errors of the estimates, their significance levels and hence the confidence that can be placed on the conclusions.

Dr Scarr-Salapatek provides no errors for her estimates of "heritability" (H statistics) and she compares and interprets these estimates with no regard to their likely errors. Elsewhere it has been argued that even data which are qualitatively adequate will not yield convincing and significant results unless sample sizes are much larger than those employed in this study⁵. It is no surprise, therefore, that when we attempt to derive standard errors for some of the comparisons made by Dr Scarr-Salapatek we find that little confidence can be placed in individual "heritability" estimates and even less upon comparisons between them.

In deriving conclusions from the raw correlations, Dr Scarr-Salapatek combines correlations firstly to estimate the intraclass correlation for monozygotic twins, (r_{mz}), secondly to estimate the "heritability", (h_r^2), and finally to compare "heritability" estimates from different subpopulations. We shall show that the tests of significance, which should be applied before strong conclusions are claimed, are practically powerless with sample sizes used in her study. Indeed, even gross effects could not be detected.

Consequences of Indirect Estimation of r_{mz}

The correlation between monozygotic twins is estimated from the z values obtained for same-sex (ss) and opposite sex (os) pairs. If the proportion of OS pairs in the population is p , then:

$$z_{ss} = \frac{pz_{os} + (1-2p)z_{mz}}{1-p}$$

giving:

$$z_{mz} = \frac{(1-p)z_{ss} - pz_{os}}{1-2p}$$

Dr Scarr-Salapatek uses r instead of z in connexion with these formulae⁷ (p. 1287), although her estimates of the MZ correla-

tions are, in fact, correctly based on the z 's. The variance of z_{mz} is given by:

$$\sigma_{z_{mz}}^2 = \left(\frac{1-p}{1-2p} \right)^2 \sigma_{z_{ss}}^2 + \left(\frac{p}{1-2p} \right)^2 \sigma_{z_{os}}^2$$

assuming p to be known exactly.

For whites p is given (p. 1288) as 0.3, which yields

$$\sigma_{z_{mz}}^2 = 3.0625\sigma_{z_{ss}}^2 + 0.5625\sigma_{z_{os}}^2$$

and for blacks ($p=0.34$):

$$\sigma_{z_{mz}}^2 = 4.2539\sigma_{z_{ss}}^2 + 1.1289\sigma_{z_{os}}^2$$

These values of σ_a^2 are inversely related to the sample sizes only. For a given number of SS and OS pairs, it is a simple matter to calculate $\sigma_{z_{mz}}^2$ since $\sigma_a^2 = 1/(N-3) = 1/N$ for large samples, where N =number of pairs. In Dr Scarr-Salapatek's samples, SS pairs are approximately twice as frequent as OS pairs, so $\sigma_{z_{ss}}^2 = 1/2N_{os} = \frac{1}{2}\sigma_{z_{os}}^2$

where N_{os} is the number of OS pairs in the sample.

Thus,

$$\sigma_{z_{mz}}^2 = 2.09\sigma_{z_{os}}^2 \text{ for whites and}$$

$$\sigma_{z_{mz}}^2 = 3.26\sigma_{z_{os}}^2 \text{ for blacks.}$$

The standard error of the restricted heritability (h_r^2) cannot be estimated directly for reasons already stated, but it is arguably pointless to produce such an estimate unless the difference $z_{mz} - z_{dz}$ is itself significant, because this difference is the numerator in the estimation of h_r^2 .

The variance of the difference is:

$$\begin{aligned} \sigma_d^2 &= \sigma_{z_{mz}}^2 + \sigma_{z_{dz}}^2 \\ &= \sigma_{z_{mz}}^2 + \sigma_{z_{os}}^2 \\ &= 3.09\sigma_{z_{os}}^2 \text{ for whites} \end{aligned}$$

$$\text{and } 4.26\sigma_{z_{os}}^2 \text{ for blacks.}$$

given samples of the same proportions as before. This estimate of σ_d^2 applies only when the indirect method of estimating r_{mz} is used. Given accurate zygosity determination on the other hand, and assuming equal numbers of monozygotic and dizygotic twins:

$$\begin{aligned} \sigma_d^2 &= \sigma_{z_{dz}}^2 + \sigma_{z_{mz}}^2 \\ &= 2\sigma_{z_{os}}^2 \end{aligned}$$

Thus, a sample of N MZ pairs and N DZ pairs gives a value of σ_d^2 which is approximately half that obtained for a sample of N OS pairs and $2N$ SS pairs of unknown zygosity. If zygosity determination is not undertaken, therefore, the size of the experiment has to be increased by a factor of, approximately, three to avoid loss of power in testing for a genetical component. This is a very damaging consequence of the indirect method of estimating the correlation between MZ twins which it may be difficult to justify on economic grounds.

Power of the Test for a Genetical Component

For a given true heritability, with certain assumptions about gene action, the mating system and environmental variation, expected values of the correlations between MZ and DZ twins can be derived. Knowledge of the standard error of the difference $z_{mz} - z_{dz}$ and the expected value of the difference enables the power of the test to be calculated for samples of a given size. That is, we can calculate for a given sample structure the probability of correctly rejecting the null hypothesis that there is no genetical component of variation. If this probability is low then the test is poor since the null hypothesis will be generally retained even though false (type II error).

There is a prior expectation that $z_{mz} \geq z_{dz}$, so the test of the difference $d = z_{mz} - z_{dz}$ is a one-tail test. That is, if

$$c = (z_{mz} - z_{dz})/\sigma_d \geq 1.65$$

we reject at the 5% level the null hypothesis that there is no heritable variation. For a given expected $z_{mz} - z_{dz}$, which depends upon the true heritability, and for a given σ_d , which depends upon the sample size, the expected value of c can be calculated, c_e . The power of the test is then the area under a normal curve with zero mean and unit variance between the limits $(1.65 - c_e)$ and infinity.

If 60% of the variation is genetically determined and there are no common environmental effects, the expected value of r_{mz} would be 0.6. This is approximately the mean value of r_{mz} given in Dr Scarr-Salapatek's study, and is an upper limit to the true broad heritability of the trait. If, further, there is no dominance, the expected value of r_{dz} will be 0.3, providing mating is at random. Under conditions of assortative mating the DZ correlation will be higher, being 0.45 if there is a correlation of 0.5 between the additive genetical deviations of spouses.

The expected value of $z_{mz} - z_{dz}$ will then be 0.3836 in a randomly mating population and 0.2084 under assortative mating of the kind just defined.

Consider the sample of upper socio-economic status (SES) whites, consisting of 70 OS pairs⁷ (Table 8, page 1291). Assume, for approximation, that the number of SS pairs, actually 155, is exactly twice that of OS pairs, so that $\sigma_a^2 = 3.09\sigma_{z_{os}}^2$ as above.

$$\begin{aligned} \text{Now } \sigma_{z_{os}}^2 &= 1/70 \\ &= 0.014286 \end{aligned}$$

$$\text{so that } \sigma_d^2 = 0.044143$$

$$\text{and } \sigma_d = 0.2101.$$

For the randomly mating population the expected value of c is thus

$$\begin{aligned} c_e &= 0.3836/0.2101 \\ &= 1.8258, \text{ when } h_b^2 = 0.6. \end{aligned}$$

The power of the test, α , is thus the area under a normal curve having zero mean and unit variance, between the limits -0.18 and infinity. In this case α can be found from tables to be 0.57. That is, a significant genetical component of variation will only be detected in randomly mating populations in 57% of all possible samples of this size, even when the broad heritability is as high as 0.6. Under conditions of assortative mating a similar calculation shows that samples of this size would only produce a significant genetical component in 25% of studies. Table 1 gives the power of the test for the four separate subclasses of Scarr-Salapatek's study by race and SES, and the value of α for tests for each race separately after pooling over social classes. The sample sizes hardly provide powerful tests of a genetical component when the subgroups are considered separately, and do not provide a very rigorous test even when a pooled "heritability" estimate is obtained for each race. It is noticeable that a moderate degree of assortative

Table 1 The Power of the Test for a Genetical Component of Variation

	Subpopulation					
	Low SES	Black High SES	Pooled	Low SES	White High SES	Pooled
Total sample size (N)	321	186	507	48	210	258
Power of test (α)						
Random mating	0.61	0.43	0.78	0.22	0.57	0.64
Assortative mating	0.27	0.20	0.37	0.14	0.25	0.29

Sample sizes approximately equal to those in Scarr-Salapatek⁷. A broad heritability (h_b^2) of 0.6 is assumed, and values are tabulated for randomly mating and assortatively mating populations. For simplicity no dominance or E_2 has been assumed.

mating reduces the power of the tests to values which would inevitably provide non-significant estimates more often than not. To provide more convincing tests (say, $\alpha=0.95$), between 800 and 1,000 pairs are needed for randomly mating populations, and between 2,000 and 3,500 pairs are needed for assortatively mating populations depending on race. If we remove the simplifying assumptions of no dominance or E_2 we find that the presence of either will tend to improve the power of the test, dominance by reducing r_{dz} relative to r_{mz} , and E_2 by increasing the overall correlation between relatives and thus, on the transformed scale, increasing the difference $z_{mz} - z_{dz}$.

Comparing "Heritabilities"

The conclusions reached so far relate only to the existence or otherwise of a genetical component of variation. We have seen that even a relatively large genetical component, corresponding to a true broad heritability (h_b^2) of 0.60, can only be detected unreliably with samples of this size. Dr Scarr-Salapatek's conclusions, however, are based on the comparison of estimates of h_b^2 for different subpopulations so we must enquire to what extent statistical unreliability is increased by attempting to draw comparative conclusions about different groups of individuals. We will concern ourselves only with a comparison between races.

The null hypothesis, that there is no racial difference in "heritability", is only rejected if the comparison $k = (z_{mz} - z_{dz})$, white; $(z_{mz} - z_{dz})$, black, differs significantly from zero.

The variance of this comparison is

$$\sigma_k^2 = \sigma_{dz}^2, \text{ white} + \sigma_{dz}^2, \text{ black}.$$

For samples in which like-sex twins are twice as frequent as unlike sex pairs

$$\sigma_k^2 = 3.09\sigma_{z_{os}}^2, \text{ white} + 4.26\sigma_{z_{os}}^2, \text{ black}.$$

There is no prior expectation about the direction of the difference so the null hypothesis will only be rejected at the 5% level if $k/\sigma_k \geq 1.96$.

With the sample sizes used in the study, $\sigma_{z_{os}}^2$, white = 0.011628 and $\sigma_{z_{os}}^2$, black = 0.005917, so that

$$\sigma_k^2 = 0.061137$$

and $\sigma_k = 0.2473$.

In an extreme case, where the true heritability in one population is 0.6, and there is no heritable variation in the other

$$c_e = 0.3836/0.2473 = 1.55$$

under conditions of random mating. The power of the test is thus the area under a normal curve of unit variance between the limits (1.96–1.55) and infinity. That is, the power of the test (α) is 0.34. Thus, even in this extreme case, we shall find, more often than not, that there is no significant difference in the genetical structure of the two populations with the sample sizes in Dr Scarr-Salapatek's study. With equal numbers of black and white pairs, nearly 4,000 pairs would be needed altogether to be 95% certain of detecting a difference of the grossest kind between the heritabilities of a trait in the two populations. If the difference is less marked, say $h_b^2 = 0.3$ in one population and 0.6 in the other, over 3,000 pairs are required for the power of such a test to be even 0.5, and upwards of 11,000 pairs would be needed before we could be 95% certain of detecting a difference between the two heritabilities. On purely theoretical grounds, therefore, we suggest that this particular experimental design, with the small samples available, could not be expected to lead to the conclusions which were drawn and indeed could only be drawn from it by omitting proper tests of significance.

The Simplest Model

We reanalysed Dr Scarr-Salapatek's data using a more rigorous approach to see whether any statistical significance could be attached to the strong conclusions she draws from the tabulated correlations. We describe an analysis of variation of the z values for the cells of a table of correlations for the two types of twin in each race and SES combination (derived from Scarr-Salapatek's Tables 6 and 7, p. 1291). We observe, firstly, that such a detailed analysis is not strictly justified by the data because the eight raw correlations are homogeneous for the non-verbal scores ($\chi_{(7)}^2 = 5.63$, $50\% < P < 75\%$) and they are barely heterogeneous for the verbal scores ($\chi_{(7)}^2 = 15.63$, $2\frac{1}{2}\% < P < 5\%$). This means that all the correlations given by Dr Scarr-Salapatek are really nothing more than estimates of the same population value of the correlation between twins, irrespective of their classification as SS or OS. We give, however, a more detailed analysis of the correlations for the verbal scores because of the slight indication of heterogeneity. The variation in the z for the eight correlations can be predicted by the linear model given in Table 2. The model includes, besides the overall mean value of z , the effects due to race, SES, and the difference between SS and OS twins. Of particular interest in the light of Dr Scarr-Salapatek's analysis, however, is the possibility of attaching tests of significance to the first order interaction between the SS/OS dichotomy and social class and that between SS/OS and race which provide the crucial tests of differences in heritability between races and social classes.

Table 2 Linear Model for Predicting the Observed Degree of Similarity between Twins (Measured by z) in Terms of Race, Social Class and Concordance for Sex

	Black				White			
	Low OS	SES SS	High OS	SES SS	Low OS	SES SS	High OS	SES SS
Mean	1	1	1	1	1	1	1	1
Race	1	1	1	1	-1	-1	-1	-1
Socio-economic status (SES)	1	1	-1	-1	1	1	-1	-1
Same sex v. opposite sex pairs (SS/OS)	1	-1	1	-1	1	-1	1	-1
Race $\times$ SS/OS	1	-1	1	-1	-1	1	-1	1
Race $\times$ SES	1	1	-1	-1	-1	-1	1	1
SES $\times$ SS/OS	1	-1	-1	1	1	-1	-1	1
Race $\times$ SES $\times$ SS/OS	1	-1	-1	1	-1	1	1	-1

Because the z values are based on different numbers of observations they do not have the same variance so the estimated components of the linear model are not orthogonal. The method of weighted least squares, however, yields maximum likelihood estimates of the effects and gives their variance-covariance matrix. These estimates are given in Table 3 with their standard errors derived from the diagonal elements of the

Table 3 Effects Contributing to Variation in the Similarity Between Twins for Verbal IQ

Effect	Estimate
Mean	0.597 *
Race	-0.048 NS
SES	0.052 NS
SS/OS	-0.069 NS
Race $\times$ SS/OS	0.025 NS
Race $\times$ SES	0.008 NS
SES $\times$ SS/OS	-0.053 NS
Race $\times$ SES $\times$ SS/OS	0.018 NS

The estimates are obtained by weighted least squares from the observed values of z . The standard error of every estimate is 0.051.

* = Significant at the 0.1% level.

NS = Not significant at the 5% level.

variance-covariance matrix. All the estimates have the same standard error since every z enters into each comparison. The fact that the only significant effect is the overall mean suggests that the slight heterogeneity of the z values cannot be assigned to any particular cause.

We find, first, that there is no significant overall difference between the correlations for SS and OS twins. This implies that the data cannot even support the well-established conclusion that there is a genetical component of individual differences in intelligence. We find further that the interactions of the SS/OS difference with race and SES are not significant. This confirms that there is no evidence that the size of any heritable component depends on race or social advantage. This finding contradicts the main conclusion of Dr Scarr-Salapatek's analysis which is based on a comparison of the numerical values of the correlations.

As there is no detectable heritable component, we cannot, on the basis of this study, suppose that the similarity between twins is due to anything other than common environmental effects. Such a conclusion is clearly inconsistent with other, more secure, evidence on this matter^{1,6,9-12}. The fact that the overall correlations for both types of twin depend neither on race nor on socio-economic status indicates that there is no difference in the magnitude of a common environmental component between the races or the two social groupings. Furthermore, the absence of a race $\times$ socio-economic status interaction implies that the magnitude of any common environmental effect does not depend on the joint effects of race and social class.

The only tenable conclusion to be drawn from the data is that there is a highly significant correlation between twins of all kinds for verbal IQ ($z=0.597$, $P=0.001$, $r=0.54$). We are in

no position to decide the cause of such similarity. There is no evidence that it has a genetical basis as far as this study goes, but as we have shown above, the likelihood of detecting such an effect with this experimental design and with these samples is very small. There is certainly no evidence in Scarr-Salapatek's studies that the proportion of genetical variation in either verbal or non-verbal IQ depends on race or social class. In view of this conclusion, and having regard to the general absence of genotype-environmental interactions for IQ^{1,13,14}, there is little justification for detailed consideration of the particular models suggested by Dr Scarr-Salapatek.

We thank Professor P. L. Broadhurst for helpful comments on the manuscript. This work is part of a research project in psychogenetics supported by the Medical Research Council.

Received August 3, 1972.

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A Quantum Mechanical Muscle Model

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Phenomena well known in chemical spectroscopy are used to explain muscular contraction; fundamental reasons for excluding alternative explanations are outlined.

THE physiology, biochemistry and structure of muscle are now well understood^{1,2} and the mechanical events that lead to contraction are being elucidated³. But how work is done with the chemical energy released on the hydrolysis of ATP remains very difficult to understand. In this paper I propose a model for muscular contraction which is of a different kind from those suggested previously and is highly speculative. I shall first outline the arguments on which the present model is based.

Elsewhere^{4,5} I suggest that the usual approach to this problem, which is to use a scaled-down version of some conventional chemical machine, is equivalent to employing a Maxwell's Demon⁶. In this case we would be forced to speculate. Fort-

unately the same argument^{4,5} sharply defines the area in which speculation is possible.

I arrive at these conclusions by first pointing out that when Szilard^{7,8} and Brillouin⁹ exorcised Maxwell's Demon they could have taken their arguments further: they could also have concluded (as has Pippard¹⁰) that the basis of the Second Law is not, after all, statistical because it is not possible to break it using a machine made of one molecule. Further, they could have concluded that the Second Law may be applied to a single molecule if we consider its time-average behaviour. But this means that a new statement of the Second Law is required because the usual definitions of Work, of Equilibrium and of Reversibility are not related to any time-scale and are therefore inapplicable at the molecular level. I then observe that the same problem exists in Molecular Biology because until we know how to distinguish a useful motion of a molecule from a merely thermal motion we will lack even the conceptual framework for solving the problems of bioenergetics. I therefore suggest the following, generally applicable, statement of the Second Law of Thermodynamics^{4,5}.

"It is impossible to devise an engine, of any size whatever, which, acting in a cycle which takes a time τ , shall produce no

effect other than the extraction of energies, which exchange with each other and reach equilibrium in a time less than τ , from a reservoir at one temperature and the conversion of these energies into a form in which they would remain stored for longer than τ ; either so that their population-distribution is characterized by a higher temperature or so that higher energy-levels are more populated than those of lower energy, that is, in a population-inversion."

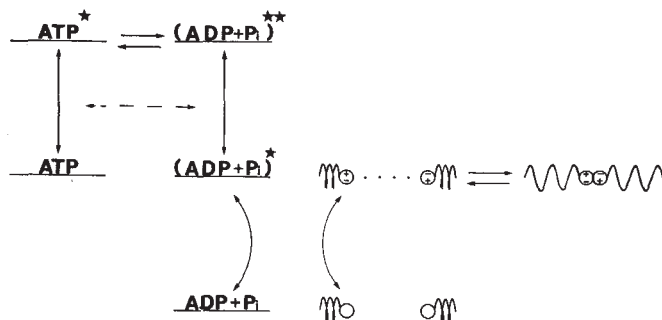


Fig. 1 Schematic energy diagram showing the transformations postulated during contraction. The star refers to a vibrational state; the $\mp$ to an electronically-excited state; the external force is represented by a mechanical spring; the dotted arrow implies energy-transfer to a different, unreacted ATP molecule and the two curved arrows imply resonant exchange between the electronic and vibrational states. Note that the total energy is conserved as the spring is stretched.

The important difference between this and previous statements of the Second Law is that both thermal energies and stored energies are defined relative to a time-interval, τ , which is characteristic of the machine under consideration. But this shows why Brownian Motion cannot do useful work and is sufficient to solve our problem. In other words we could define "doing useful work" as the conversion of one form of stored energy into another.

However, in those biological processes such as muscular contraction, active transport, and oxidative phosphorylation, where useful work is done, it is done molecularly; the organelles that do the unit process act cyclically and use, or produce, one molecule of ATP at a time¹⁴. A single organelle (in contrast to a Maxwell's Demon) can do useful work. It follows that the energy released by the hydrolysis of ATP must have been stored in the molecule itself. But the above statement of the Second Law shows that if energy is stored in a single molecule but is allowed to exchange with thermal energies then it becomes mere heat and is thereafter useless to that system. The crux of my argument is that while this is well known, even trivial, in the case of energy stored in a molecule in an excited state, it has not been recognized before that it is just as true of the energy stored in a single molecule of ATP. Hence we must conclude that when ATP-energy is used to displace an external force it must never have been allowed to exchange with thermal energies during the process. Yet in all conventional chemical machines, from steam-engines to fuel-cells, the energy is not stored in molecules individually but in the system as a whole and, at the step where useful work is done, the molecules are as close to thermal equilibrium as practicable; thermal exchange is the essence of the mechanism^{4,5}. Hence a muscle must use a completely different kind of mechanism.

Possible Solution

If we reconsider the definition of "doing work" given above, it is clear that the alternative to doing it close to thermal equilibrium (as slowly as possible) is to do it as rapidly as possible, but before thermal exchange can occur. In other words, if

stored energy could be released in some specific molecular form and then converted into another form so quickly that it is available for thermal decay for a negligible time, then useful work could be done with it. This we could call a "Molecular Energy" mechanism.

Consider how a macroscopic effect could be produced using such a mechanism. In the general sense used above a solution of ATP in water (which normally hydrolyses slowly) is in a metastable population-inversion relative to its hydrolysis-products. In this case nothing is needed to prevent the state decaying but a specific action, distinct from thermal energy, is needed to release the energy stored in this way.

For maximum efficiency the muscle-projection³ must act only when it touches the thin filament. Clearly, we can postulate some conformational change to lower the activation energy on binding, but this alone is no answer unless, at the same time, the reaction is constrained to prevent wastage. If we are not careful we begin an infinite regression^{4,5}.

Activation energy, however, is usually obtained from thermal collisions and there is evidence that in complex reactions it takes a specific form¹². Let us suppose therefore that there is a specific vibration of the ATP molecule which provides the activation energy for hydrolysis when it is excited. Suppose, however, that, immediately after reaction, two vibrations of this frequency are produced, the energy for one remaining after the activation and for the other coming from the chemical energy released. The vibration would then be amplified (Fig. 1, left hand side). The hydrolysis of ATP is exothermic with an enthalpy of about 40 kJ/mol¹², hence the wavelength of the corresponding radiation would be about 3 μ m.

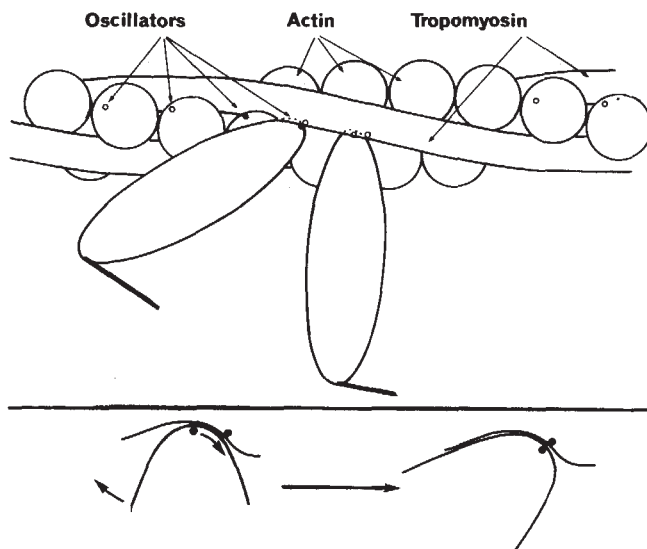


Fig. 2 A possible way to couple the excimer-state with contraction. This shows how a molecular energy mechanism could be used to operate H. E. Huxley's model². The function of the tropomyosin and of the oscillators is explained in the text.

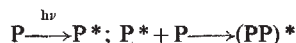
The shortest time in which a projection can act¹⁴ appears to be about 10^{-3} s, but this is a far longer time than required for the relaxation of a vibration¹⁵ (about 10^{-7} s). Nevertheless it is possible in principle to make use of such a vibration because resonant processes are even faster ($\sim 10^{-13}$ s). For example the HCl produced by the reaction between H_2 and Cl_2 is initially vibrating and it has been possible to convert some of the energy of this reaction into coherent infrared radiation in a chemical laser¹⁶. Let us suppose therefore that the postulated vibration exactly resonates with a vibration in a suitable protein. This protein could then be used to spread the reaction auto-catalytically in the following way.

Tropomyosin is present in the thin filaments¹⁷; it may be entirely α -helical¹⁸; it is a long enough molecule to extend between two projections on the thick filament. Because the α -helix has a strong resonance at about 3 μm (ref. 19) let us suppose that the function of tropomyosin is to conduct the vibration so that the ATP hydrolysis spreads only to those projections which overlap the thin filaments. This would prevent wasteful reaction. Moreover, suppose that the function of troponin¹⁷, which is found in association with tropomyosin and sensitizes actomyosin to Ca ions, is to ensure that this conduction is only allowed in the presence of Ca ions. Then, after an influx of Ca ions, there would be a latent period until one projection reacted spontaneously and then the reaction would spread to the appropriate projections like a controlled explosion. The next step is to trap the excitation and produce a force.

Suppose therefore that there is present in the projection a chemical moiety which has an electronic excited state which also exactly resonates with the vibration. Let us call this the "oscillator". Further, suppose that there is an identical oscillator in the thin filament. If it was arranged that the ATP only "saw" the activating vibrations in the thin filament through the oscillators, because this could only occur by resonance and therefore at a particular separation (see below), then automatically reaction would only take place when the projection is ideally positioned for its subsequent purpose. The oscillators would then become excited and share an excitation. One possible way of achieving this is shown in Fig. 2. The mechanism by which a force is generated is given below. It is clear that although he did not suggest a specific mechanism, Szent-Györgyi was thinking along these lines²⁰.

Molecular Constraint

When a concentrated solution of pyrene is irradiated "excimers" are formed²¹. Förster has accounted for these in terms of quantum-mechanical resonance between an excited molecule and another in its ground-state:



An excited molecule can be considered simply as an oscillating dipole²², and an excitation is shared between two identical oscillators if their dipoles are coupled π out of phase. They then attract each other and get closer. As this takes place the coulombic attraction of the two dipoles decreases the energy of the excited state. Therefore the energy of the excitation is first converted into kinetic energy and then into heat. As a result the observed fluorescence is redshifted²¹, relative to the unperturbed fluorescence at lower concentrations. Moreover, the half-life of an excimer is somewhat greater than that of the unperturbed singlet state²³. But this is so because an excimer has no net dipolar field (triplet states have long half-lives for the same reason). Hence we can predict that, in the same way that the triplet state of chrysene can be stabilized in a plastic matrix at room temperature for as long as a second²⁴, it should be possible to stabilize an excimer in a suitable environment: for example in a protein evolved for this purpose.

I propose therefore that the two oscillators, in the projection and the thin filament, couple when excited to form a long-lived excimer state and thus experience a force. This force is opposed by the external tension, which is then displaced so that the energy of the excitation (released originally from the ATP) is converted directly into external work. As soon as movement begins, the excimer state would have lost energy and hence could no longer resonate with the vibration-frequency of the tropomyosin. In this case the energy would become trapped in the excimer state and remain there until it was used up. This process is shown schematically in Fig. 1. When the oscillators get close enough the residual energy would become no greater than kT and hence the excitation would

become liable to decay by thermal collisions. The projection would then be released, ready for refuelling for the next cycle.

If we approximate the dipoles of the oscillators by unit electronic charges separated by 0.1 nm and suppose that the oscillators are initially 3.0 nm apart and approach to 0.3 nm, then, using the classical equations for the force and energy shared between two dipoles²², we obtain 3×10^{-4} dynes and 50 kJ/mol respectively. These are of the required order of magnitude¹¹.

Two Important Consequences

In the above model the coupling is electromagnetic and the energies are stored. Therefore the mechanism is necessarily reversible: if the excimers are pulled apart, useful work would be put into the state instead. Consequently it should be possible in principle, first, to make muscles oscillate mechanically and, second, to resynthesize ATP by overloading the muscle. Pringle¹⁴ has shown that the first occurs naturally in asynchronous insect flight muscles and Hill²⁵ has suggested the second. These possibilities are not *ad hoc*; they are a necessary property of the model.

Elsewhere^{4,5,26} I have expanded the above arguments and considered other bioenergetic processes. The model has been discussed more fully and experimental tests outlined. The Hill equation can be derived from this model in as simple a manner, and for much the same reason, as Hill originally suggested²⁷.

I thank Professor Sir John Randall for provision of facilities and Professor Sir Karl Popper, Professors E. J. Hanson, D. R. Wilkie, E. W. Taylor, C. Domb, and D. Bohm, and Drs G. W. Offer, E. G. Richards, D. M. Engelman, and David Baxter, for valuable criticism and discussions. D. E. Hookes contributed to the writing of the section on molecular constraint.

Received June 1969; revised October 21, 1970.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Sunspots and Planets

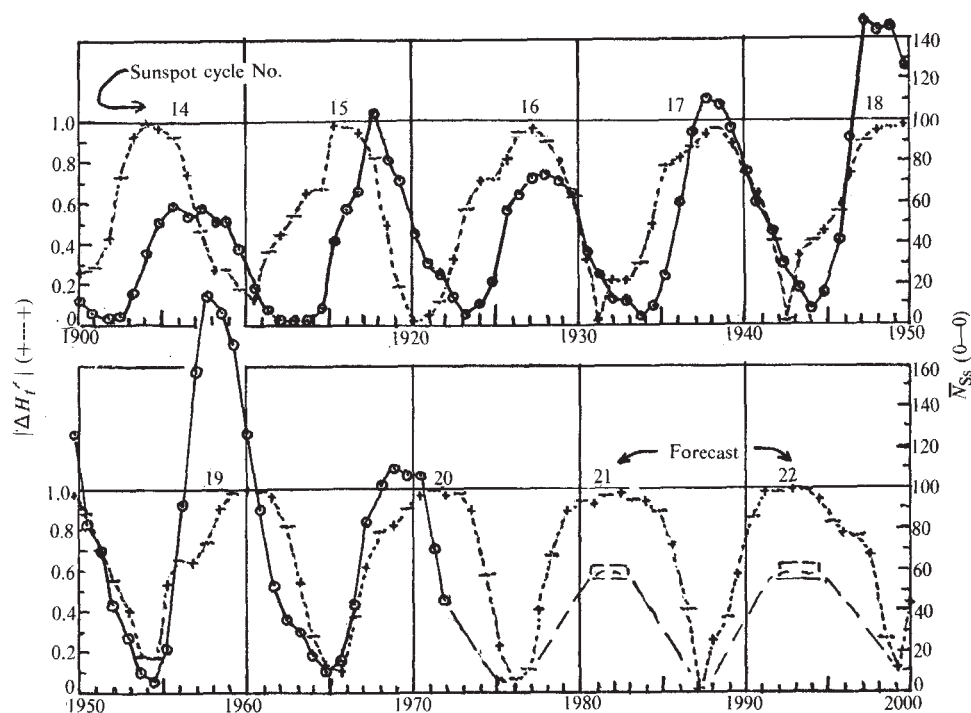
Most current literature on solar activity assumes that the planets do not affect it, and that conditions internal to the Sun are primarily responsible for the solar cycle. Bigg¹, however, has shown that the period of Mercury's orbit appears in the sunspot data, and that the influence of Mercury depends on the phases of Venus, Earth, and Jupiter. These four are the "tidal planets"; their relative tidal heights raised on the Sun are Mercury 1.15 ± 0.65 , Venus 2.17 ± 0.04 , Earth 1.00 ± 0.05 , and Jupiter 2.28 ± 0.32 . The ranges of values are due to the orbit eccentricities. No other planet raises a solar tide greater than 2% of the Earth's contribution. Here I discuss

Earth-Venus conjunctions occur every 1.60 yr. During the 0.8 yr before the subsequent opposition the Earth moves relative to Jupiter by $264^\circ \pm \text{about } 10^\circ$, depending on where the planets are in their orbits relative to their perihelia. Thus, starting with a conjunction in the direction $\varphi_{rc} = \varphi_J - \varphi_{EVo} = 0$, the next opposition is in a direction φ_{EVo} such that $\varphi_{ro} = \varphi_J - \varphi_{EVo} = 264^\circ \pm 10^\circ$, which is nearly, and sometimes exactly, 270° . When $\varphi_{ro} = 270^\circ$, the Earth-Venus opposition is at right angles to the direction of Jupiter. Tides at right angles to each other tend to cancel each other near the solar equator, though they still reinforce each other at solar latitudes greater than about $55^\circ (= \cos^{-1} \sqrt{1/3})$, neglecting the inclination of about 7° of the solar axis to the mean plane of the planets.

The basic tidal height equation² is

$$H_t = 1.5 H_{tm} (\cos^2 \theta - 1/3) \quad (1)$$

Fig. 1 Tidal height fluctuation ($|\Delta H_t|$) and sunspot number ($\bar{N}_{ss}$) twentieth century.



the relationships between planetary tides on the Sun and number of sunspots. The effects attributed to Mercury have been neglected because of their very short period of about 3 months compared with the period of the Sunspot cycle, which averages about 11.1 yr.

When Earth and Venus are in either conjunction or opposition, they raise a tide on the Sun which is about 50% larger than the largest tide caused by Jupiter. When the direction of an Earth-Venus conjunction or opposition φ_{EV} is in the direction of Jupiter φ_J , so that $\varphi_r = \varphi_J - \varphi_{EV} = 0$, the tidal effects of the three planets are added at all points on the Sun.

where H_t is the tidal height in any direction θ measured between the line from the centre of the Sun to the direction of the planet and any point on the Sun's surface, and H_{tm} is the maximum tidal height due to the planet (directly "under" the planet, at $\theta = 0$). When $\theta = \varphi_r$, equation (1) gives the tidal height in the direction of Jupiter caused by an EVc or EVo. The change in tidal height in the direction of Jupiter between an EVc and an EVo is, if $H_{tmc} = H_{tmo}$ (which is usually true within a few per cent)

$$\Delta H_t = H_{tmc} (\cos^2 \varphi_{rc} - \cos^2 \varphi_{ro}) \quad (2)$$

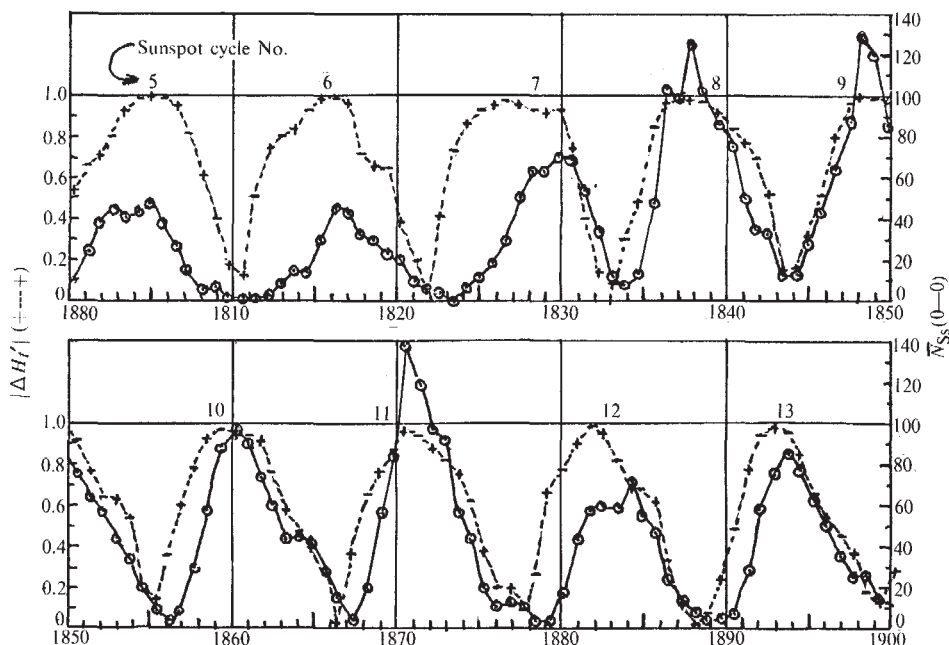
To non-dimensionalize, I define $\Delta H_t' = \Delta H_t / H_{tmc}$, the "tidal fluctuation".

I have calculated the tidal fluctuation for each interval between dates of EVc and EVo from 1750 to 1972, and compared this with the smoothed monthly number of sunspots at the middle of the interval, $\bar{N}_{ss}$ (Figs. 1 and 2). Planet position data were from HM Nautical Almanac Office³ and from Tuckerman⁴; sunspot data from Waldmeier⁵. The plots go back only to 1800 because Waldmeier has stated that the consistency of the relation between earlier and more recent data "may be doubted", although there is no doubt about the peak and valley dates, as discussed later.

stated; the median ϕ_r is $0 \pm 1^\circ$, and 1° is exactly the uncertainty of the data, due to inaccuracies of interpolation and extrapolation. For the middle 50% of the sunspot peak data, ϕ_r was less than $\pm 9^\circ$ from zero.

Because small differences in ϕ_r sometimes caused large differences in dates of tidal maxima, I found it useful to define a "peak region" where ϕ_r is less than about 20° , and to use the mid-date of this region as another measure of the date of maximum tidal fluctuation. A date halfway between the date or ϕ_r minimum and the mid-date of the peak region is the "tidal peak" date shown in Table 1, together with sunspot cycle peak dates similarly determined. Some of the sunspot

Fig. 2 Tidal height fluctuation ($|\Delta H_t'|$) and sunspot number ($\bar{N}_{ss}$) nineteenth century.



In Figs. 1 and 2, successive changes $\Delta H_t'$ are usually of opposite signs; the actual signs are plotted + or - on the graphs; the absolute values $|\Delta H_t'|$ are the numerical values plotted. Inspection of Figs. 1 and 2 shows obvious high correlation between $|\Delta H_t'|$ and $\bar{N}_{ss}$ for eight cycles (5, 8, 9, 10, 11, 13, 18); for cycle 18 the correlation coefficient exceeds 0.9. For all cycles the peak dates and valley dates agree well, but for five cycles (7, 12, 14, 15, 17) the sunspot rise seems to lag the tidal rise by several years. The best physical explanation is not, I believe, a delayed tidal effect, but either some non-tidal factor contributing to the formation of sunspots or some tidal effect not considered in this simple calculation.

Sunspot cycle peak and valley dates are available as far back as AD 1600. Study of the mismatch between tidal and sunspot peak and valley dates does not require calculation of $|\Delta H_t'|$ for all intervals between EVc and EVo because peaks of $|\Delta H_t'|$ occur whenever ϕ_{rc} or ϕ_{ro} is a minimum, measured from any of the four rectangular axes, and valleys occur whenever ϕ_{rc} or ϕ_{ro} is in the region from 35° to 45° to these axes. Accordingly, I have calculated the angle ϕ_r for each of 625 dates of EVc and 625 dates of EVo from AD 1600 to AD 2100 and determined the dates of minimum and maximum ϕ_r by inspection of the resulting tables (Tables 1 and 2). Sunspot cycle peak and valley dates are also shown, and the differences, usually less than 1 yr and nearly always less than 3 yr, are tabulated as E_{pp} (=error of prediction of peak date) and E_{pv} (=error of prediction of valley date). For the sunspot peak data in Table 1, ϕ_r at the date of sunspot peak was also calculated but is not shown because the results may be simply

peak dates in Table 1 thus differ from the dates published by Waldmeier⁵ after AD 1750, but the difference is usually only a fraction of a year.

The distribution of the 34 values of E_{pp} in Table 1 is as follows: median -0.3 yr; middle 53% (18 values) from -1.5 to +0.9; middle 41% (14 values) from -1.0 to +0.8. The "probable error" (or half the range of the middle 50%) is thus between 0.9 and 1.2 yr.

The sunspot and tidal valley data in Table 2 were handled much the same as the peak data of Table 1, except that ϕ_r maximum dates with ϕ_{rmax} (always less than 45°) are not identified as tidal minima, which equation (1) shows to be 35.3° . Table 2 lists the values of ϕ_r at the actual sunspot minimum dates, and these are found to have a median value of 36° , with the middle 50% within $\pm 3^\circ$ of 36° . Chance would give $\phi_r = 22.5^\circ$ at sunspot minimum.

The distribution of the 33 values of E_{pv} in Table 2 is as follows: median +0.8 yr; middle 51.5% (17 values) from -0.3 to +1.1 yr; probable error = 0.7 yr, substantially less than the ~1 yr probable error of E_{pp} .

The maximum smoothed monthly number of sunspots for each solar cycle, N_{ssPk} , from Waldmeier's data, is also given in Table 1. When N_{ssPk} is plotted against date, the curve appears to repeat after about 170 or 180 yr, but with only 220 yr of data and some doubt about the consistency of earlier and more recent data, one can hardly be sure of this.

When E_{pp} in Table 1 is plotted against date, the curve follows closely the graph of N_{ssPk} against date. A graph of E_{pp} against N_{ssPk} shows a close linear relationship, with a

correlation coefficient of about 0.9 for all cycles since 1820, although the correlation is substantially lower if the less certain data back to 1750 are included.

Table 1 includes a forecast of peak dates and sunspot numbers for the next 130 yr (12 sunspot cycles). The tidal peak dates are as certain as the future positions of the planets. For future sunspot peak date estimates, I assumed that E_{pp} varies with time with the same 170 yr to 180 yr cycle in the future as it has in the past. Because of the past data in Table 1, I believe that the estimated future sunspot peak dates have a probable error of substantially less than one year (but of course, in view of the errors of cycles 3 and 4, possible errors of several years).

Table 1 Comparison of Sunspot Cycle Peak Dates with Dates of Peak Planetary Tidal Fluctuation

Sunspot cycle No.	Sunspot peak date	Tidal peak date	E_{pp}	N_{ss} at peak
-13	1604.5	1605.4	-0.9	No data
-12	1615.5	1617.5	-2.0	
-11	1626.0	1628.3	-2.3	
-10	1639.7	1639.5	+0.2	
-9	1649.0	1650.5	-1.5	
Uranus-Neptune period 1				
-8	1660.0	1661.5	-1.5	No data
-7	1675.0	1672.1	+2.9	
-6	1685.0	1683.3	+1.7	
-5	1693.0	1693.7	-0.7	
-4	1705.5	1705.1	+0.4	
-3	1718.2	1716.2	+2.0	
-2	1727.5	1727.0	+0.5	
-1	1738.7	1738.4	+0.3	
0	1750.1	1749.8	+0.3	92
1	1761.5	1760.6	+0.9	86
2	1769.8	1772.2	-2.4	116
3	1778.5	1782.8	-4.3	158
4	1788.2	1794.1	-5.9	141
5	1804.8	1805.6	-0.8	49
6	1816.4	1816.6	-0.2	49
Uranus-Neptune period 2				
7	1830.0	1827.0	+3.0	72
8	1837.2	1838.2	-1.0	147
9	1848.3	1848.7	-0.4	132
10	1860.2	1859.5	+0.7	98
11	1870.7	1871.0	-0.3	140
12	1884.0	1881.0	+3.0	75
13	1893.9	1893.1	+0.8	88
14	1906.3	1904.5	+1.8	63
15	1917.7	1915.9	+1.8	105
16	1928.4	1927.1	+1.3	78
17	1937.6	1937.9	-0.3	119
18	1947.5	1949.5	-2.0	152
19	1958.2	1960.7	-2.5	201
20	1969.3	1971.3	-2.0	110
Forecast *				
21	1982.0	1982.0	0	60
22	1993.4	1992.9	+0.5	60
Uranus-Neptune period 3				
23	2002.1	2004.1	-2.0	80
24	2014.0	2014.5	-0.5	130
25	2025.6	2026.1	-0.5	130
26	2036.1	2037.6	-1.5	90
27	2048.3	2048.3	0	120
28	2056.6	2059.1	-2.5	70
29	2071.4	2070.9	+0.5	90
30	2079.5	2081.5	-2.0	70
31	2093.6	2092.6	+1.0	100
32	2104.4	2103.9	+0.5	90

* Assuming E_{pp} and N_{ssPk} repeat about every 170 to 180 yr in the future as in the past. Data above are grouped by Uranus-Neptune synodic periods but other physical factors may be more significant.

Average cycle lengths are: sunspots 11.05 yr, planet tides 11.08 yr.

Table 2 Comparison of Sunspot Cycle Valley Dates with Dates of Maximum Relative Angle ϕ_r and Values of ϕ_r at $N_{ss\ min}$

Cycle No.	$N_{ss\ min}$ date	ϕ_r max date	E_{pp}	ϕ_r at $N_{ss\ min}$
-13, -12	1610.8	1610.3	+0.5	36°
-12, -11	1619.0	1621.7	-2.7	12°b
-11, -10	1634.0	1633.1	+0.9	38°
-10, -9	1645.0	1644.3	+0.7	34°
-9, -8	1655.0	1655.7	-0.7	36°b
Uranus-Neptune period 1				
-8, -7	1666.0	1666.7	-0.7	33°b
-7, -6	1679.5	1678.1	+1.4	30°
-6, -5	1689.5	1688.9	+0.6	42°
-5, -4	1698.0	1700.3	-2.3	24°
-4, -3	1712.0	1710.9	+1.1	39°
-3, -2	1723.5	1722.5	+1.0	37°
-2, -1	1734.0	1733.0	+1.0	43°
-1, 0	1745.0	1744.0	+1.0	37°
0, 1	1755.2	1755.0	+0.2	40°
1, 2	1766.5	1765.6	+0.9	40°
2, 3	1775.4	1771.1	-1.6	36°
3, 4	1784.4	1787.8	-3.1	15°b
4, 5	1798.4	1798.9	-0.5	36°b
5, 6	1811.0	1810.2	+0.8	40°
Uranus-Neptune period 2				
6, 7	1823.2	1821.4	+1.8	23°
7, 8	1833.9	1833.0	+0.9	35°
8, 9	1843.7	1844.0	-0.3	41°b
9, 10	1856.0	1855.2	+0.8	35°
10, 11	1867.2	1866.1	+1.1	36°
11, 12	1878.9	1877.5	+1.4	29°
12, 13	1890.1	1888.5	+0.6	35°
13, 14	1901.5	1899.1	+2.4	35°
14, 15	1913.3	1910.1	+3.2	27°
15, 16	1923.5	1920.9	+2.6	25°
16, 17	1933.8	1931.7	+2.1	36°
17, 18	1944.2	1942.9	+1.3	33°
18, 19	1954.4	1953.9	+0.5	40°
19, 20	1964.8	1965.3	-0.5	41°b
Forecast *				
20, 21	1977.0	1976.2	+0.2	35°
21, 22	1988.8	1987.6	+1.4	35°
22, 23	1997.7	1999.0	+1.0	35°

* Assuming mean E_{pp} follows previous pattern about every 170 to 180 yr, and $\phi_r = 35^\circ$ at minimum N_{ss} .

All angles ϕ_r are after $\phi_r\ max$ except those labelled b (= before).

The forecast values of peak sunspot number N_{ssPk} shown in Table 1 also assume that the 170 to 180 yr cycle pattern of the past repeats in the future. Because the past mismatch averages about 15 smoothed monthly sunspots, I estimate that the probable error of the N_{ssPk} forecast is about 15.

Table 2 includes forecast dates for the next three tidal valleys, using a procedure similar to that described for peaks.

I thank Dr R. M. Wood for help in the preparation of this paper.

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Received April 17; revised August 14, 1972.

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Implications of Lunar Aseismicity

THE aseismicity of the Moon has been interpreted¹ as an indication that the Moon is relatively cold in the interior. More precisely, the temperature $T(z)$ at depth z is inferred to be so low that the strength $\sigma_{\max}(T, \tau)$ of the rock over the appropriate time scale τ is greater than the ambient shear stress $\sigma(z, \tau)$, so that no displacement occurs. (The time scales range from weeks for tidal stresses to aeons for mascon stresses.) Unfortunately, we have only a poor understanding of the long term strength $\sigma_{\max}$ and of the fields of stress. Hence this argument can put only rough upper limits on the temperature profile; it is definitely below the solidus, and probably considerably below it.

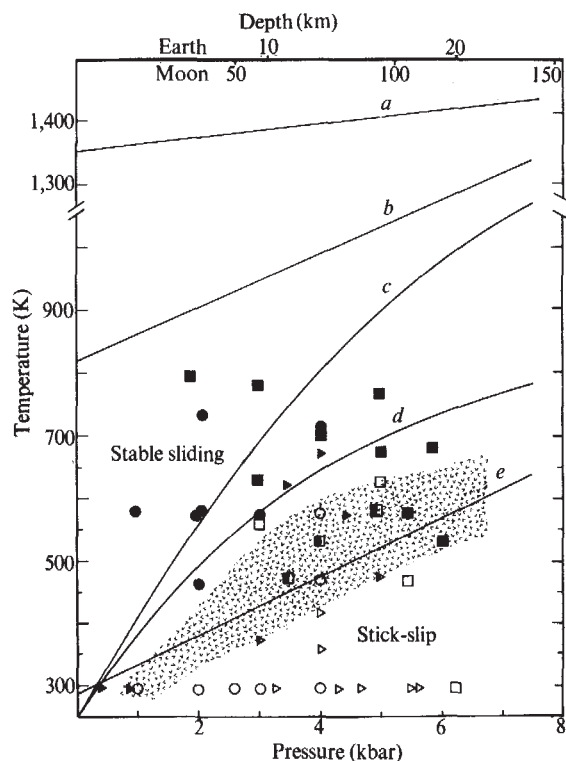


Fig. 1 The modes of failure under shear stress of two terrestrial rocks⁴, compared with possible temperature profiles in Earth⁵ and Moon (Hanks and Anderson, unpublished work), and two relevant phase lines of basalt¹¹. The solid, open, and half-open symbols locate experiments where the failure occurred by stable sliding, stick-slip, and intermediate behaviour, respectively. The shaded area indicates the zone of transition between the two principal types of failure. The field of stable sliding would be enhanced by slower straining rates, greater porosity, presence of weak (such as hydrous) minerals, or presence of pore fluids (following an effective stress law). The lunar-therms would be raised by a lower thermal conductivity, and lowered by a more extreme upward concentration of radioactivity. Gabbro: $\blacktriangle$, faults; $\bullet$, sawcuts. Granite: $\blacksquare$, faults. a, Dry basalts solidus. b, Gabbro eclogite. c, Moon (2b). d, Moon, (2c). e, Earth.

Here I present an alternative argument which avoids any assumptions or conclusions on strength properties or stress levels, and which suggests the reverse conclusion, that the Moon is too warm to be seismic. The argument is based on known properties of materials, in particular their modes of failure under shear stress. It has been argued² that stick-slip failure of rock is a viable analogy of an earthquake and this analogy has been used recently³ to explain the lack of seismicity deeper than 15 km in California. In accepting such an analogy it is well to bear in mind the caveats stated^{3,4} by the experimenters themselves. If, however, the analogy is accepted, and further extended to moonquakes, a simple explanation of

lunar aseismicity follows. The Moon is aseismic because its temperature-pressure curve lies in the P - T field where rock fails by stable sliding rather than by stick-slipping.

This explanation is supported by Fig. 1, where the modes of failure (along pre-existing surfaces) of terrestrial granite and gabbro fall into two fields (stick-slip and stable sliding) separated by a narrow transition zone. The geotherm lies close to the marked transition zone, depending on local conditions in the Earth's crust; a reasonable model⁵ is shown. The P - T locus of the transition zone depends strongly on such factors as the mineralogy, porosity, pore pressure, gouge thickness, etc., of the specimen⁴; hence the relative positions of geotherm and transition zone may be expected to differ greatly at different localities and depths in the Earth.

On the other hand, the lunar-therm may be expected to lie well within the field of stable sliding. This expectation is based upon the observed Apollo 15 heat-flow measurement⁶, which is not atypical of heat-flow on the Earth. Hence, except for differences in conductivity and radioactivity distribution between the two bodies, the temperatures at shallow depths will be similar (although this similarity rapidly becomes uncertain with depth, owing to these differences). Because of the lower gravity on the Moon, the pressure at a given depth will be six times less in the Moon than in the Earth, hence a typical lunar-therm will have a P - T slope much smaller than that of a typical geotherm, and may be expected to lie well within the field of stable-sliding, as shown in the figure.

These expectations are supported by model calculations; the lunar-therms shown in the figure were supplied by Hanks and Anderson (unpublished work). Despite uncertainties in the calculation, the two curves are regarded respectively as quasi-lower-limit (2c) and more-likely (2b) estimates of the lunar-therm. They correspond to concentrations of the radioactivity into the top 50 km and 110 km, respectively. It is possible, however, to find thermal models which predict temperatures in the stick-slip field, for example by the concentration of sources into the top 10 km⁷; such extreme differentiation would be surprising.

Hence the tentative conclusion follows that the Moon is largely aseismic because the lunar-therm lies in the stable-sliding (high-temperature) field of rock failure; it is too warm, rather than too cold. It need not be hot, because the transition zone to stable-sliding behaviour is far below the solidus (Fig. 1).

The observed⁸ moonquakes at apparently great depth (800 km) are not directly included in the present argument because this depth corresponds to 34 kbar, far beyond the experimental range shown in the figure. They could be fit *ad hoc* into the present picture by postulating that the lunar-therm crosses through the stick-slip field at this depth.

Nor does the present argument address directly the shallowest depths ($T < 300^\circ$) or the fact that the Moon's figure is slightly non-hydrostatic. The experimental data in Fig. 1 define fields of failure-mode, not fields of stress-level at failure; these two properties are clearly distinct, although they may be related in ways which are not yet clearly understood. (In fact, the strengths of these specimens shown in Fig. 1 were of the order of kilobar, rather than tens of bars, but this result may well be due to the rate of shearing ($\dot{\eta} = 10^{-4}$ to 10^{-6} s⁻¹) in the experiments, which may differ by many orders of magnitude from the lowest strain rates on the Moon.) The apparent persistence over long times of the lunar mascons (with their associated stresses⁹ of about 50 bar over roughly 80 km depth) presumably does imply that locally the long-term strength is sufficient to support these stresses, hence no straining, hence no earthquakes. Experiments show that strength of this magnitude is quite consistent with failure by stable sliding in response to stresses greater than this. Any implications of the existence of mascons for tectonic stresses or rheological properties at lower depths, or at different time scales (such as tidal), are very speculative. The present argument is that, irrespective of the stress levels, the lunar rock, if secularly strained, would do so by flowing rather than by breaking,

I thank W. F. Brace, T. C. Hanks, D. A. Papanastassiou and M. N. Toksöz for helpful discussion. This work was supported by NASA.

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Received May 15; revised September 18, 1972.

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Volatilization Losses from Lunar Lava 14310

THE recent debate^{1,2} over alleged volatilization losses from lunar sample 14310 is fundamental to the interpretation of lunar petrogenesis. If alkalis, water and oxygen have been lost in significant amounts from lunar lavas, it would eliminate from consideration one class of interpretations which presuppose the cooled rock compositions are directly indicative of the chemical constitution of the lunar interior, and hence of a particular model of lunar origin.

The lunar lavas were erupted as liquids at 1,100–1,300° C (refs. 3, 4) into a vacuum of 10⁻¹⁰ torr or harder. Several components develop vapour pressures many orders of magnitude greater than this when the present rock compositions are heated to these temperatures⁵; further, mass transfer rates have been observed for the alkalis which are sufficiently high to guarantee significant losses during the likely eruption processes⁶. It would be remarkable if eruption had taken place under such conditions without selective volatilization losses.

Green *et al.*² state incorrectly that volatilization was originally advanced to explain the absence of plagioclase on the liquidus of mare-type basalts; it was advanced³ as a factor invalidating simplistic interpretations of lunar sample petrogenesis solely because it seemed common sense, having due regard to the environment and circumstances of eruption. Plagioclase is, moreover, present at or very close to the liquidus of the parental magmas erupted at the Apollo 11 and 12 sites^{6,7}; much confusion has been caused by interpretation of data obtained for exceptional specimens enriched by accumulation of dense ferromagnesian phenocrysts, without due regard to the average lava composition erupted at the sites, and by the use of uncontrolled and subjective visual estimates of crystallinity and crystal species ratios which are grossly discrepant from our quantitative results based upon reversible equilibrium experiments at known and controlled oxygen fugacity⁶.

The present composition of 14310 has been depleted by about 0.7% weight of oxygen since the start of crystallization of its ferromagnesian phases⁸. Experiments carried out on the material in sealed capsules yield iron droplets, which are not present in the natural rock, and a pyroxene significantly poorer in oxidized iron than that actually observed in the natural rock (approximately Mg₈₂Fe₁₇ instead of Mg₈₈Fe₁₂).

The oxygen fugacity appropriate to the mineralogy of this rock at its liquidus temperature is approximately the same as the confining pressure on the lunar surface. It is inescapable that oxygen at least was selectively volatilized from this magma after its eruption.

Oxygen is, however, much less volatile from silicate melts than either the alkalis or water. The possibility of significant selective volatilization of alkalis and water is thereby established, and neither the present low oxygen fugacity, nor the water-poor character, nor the alkali depleted nature of the lunar surface rocks can yet, in common sense, be ascribed to the Moon as a whole.

All estimates indicate that the mantle of the Moon, like that of the terrestrial planets, is largely composed of ferromagnesian silicate minerals. There are no known conditions under which such materials can be partially melted in the absence of water to yield liquids as anorthite-rich as the 14310 magma, or the lunar highland basalts. In the presence of saturation water vapour pressures, however, ferromagnesian silicates are in equilibrium with anorthositic liquids⁹. The composition of the lunar crust is very much that expected of the initial partial melting products of a water-rich Moon. Volcanism at low confining pressures involving water-rich magmas of this type would inevitably be accompanied by extensive dispersion of liquid droplets, creating very large surface areas of liquid from which selective volatilization would have occurred. When sample 14310 is crystallized in the presence of water vapour at quite low pressures, olivine, spinel and anorthite can be induced to precipitate almost simultaneously at the liquidus⁷. Fragments of olivine-spinel-anorthite rocks (spinel troctolites) with igneous accumulate textures are reported from Fra Mauro formation breccias at the Apollo 14 site¹⁰⁻¹², strongly supporting the interpretation that water played a significant role in the evolution of 14310 and other rocks from the lunar highlands. In the dry state, neither 14310 nor any other magma in evidence from the lunar collections could precipitate a spinel-troctolite accumulate.

Brown and Peckett¹ concluded that a large proportion of the alkalis originally present in the 14310 magma were lost after the formation of the large feldspar phenocrysts and before the crystallization of the groundmass feldspar. Their evidence was that the phenocryst cores of An₉₅ were zoned outwards to An₆₇, while the groundmass feldspar was entirely An₉₃.

Green *et al.*² opposed this interpretation on the grounds that in their experiments at low pressure the liquidus plagioclase, which formed from the present 14310 bulk composition, was An₉₂Or_{0.5}, whereas if alkalis were restored towards the composition as suggested by Brown and Peckett, much more sodic feldspar, not resembling the An₉₅Or_{0.5} cores to the phenocrysts, would have been the first feldspar to crystallize. Their counter-interpretation, however, suffers from inconsistent logic. The most calcic experimental liquidus plagioclase in the 14310 composition is An₉₂ (at 1,320° C) whereas the phenocryst cores in the natural rock are An₉₅ (ref. 2). Elsewhere¹³ the same authors argue that comparable differences in olivine compositions are significant. It follows, if the logic displayed was correct, that the present 14310 composition is not that of the magma which precipitated the plagioclase phenocrysts. The counter-interpretation², moreover, does not satisfactorily explain either why the sodic rims (An₇₁) crystallized on the large phenocryst cores only, ignoring the much larger surface area offered by the groundmass plagioclase crystals, or why olivine and aluminous spinel, which are conspicuous in the low pressure experimental crystallization sequences of 14310 between 1,240° C and about 1,200° C, are not in evidence in the natural rock⁷. Finally the counter-interpretation² is cogent only if there has been no water loss from the magma on eruption; but the probability and part of the petrological evidence for significant water loss from 14310 have been established above. The remainder of the evidence comes from the petrography of 14310 itself, the correct interpretation of which we believe to be as set out below.

Table 1 Comparison of Compositions of Basalt Cinders and Lunar Sample 14310

	SiO ₂	TiO ₂	Al ₂ O ₃	Cr ₂ O ₃	Fe ₂ O ₃	FeO	MnO	MgO	CaO	Na ₂ O	K ₂ O	P ₂ O ₅	H ₂ O+
A	47.9	0.83	19.3	n.d.	4.59	4.74	0.17	6.59	12.43	2.31	0.34	0.08	0.49
B	46.4	1.22	19.8	0.18	4.37	4.31	0.11	7.75	12.10	2.39	0.48	0.33	0.49

A is basalt cinders 14802 from Mansion pyroclastics, St Kitts (15); B is 14310 (16) recalculated after addition of 0.44% O₂, 0.50% H₂O and 1.80% Na₂O.

Both magmas are suspected to have precipitated An₉₀₋₉₅ at moderate water vapour pressure, An₇₀ on eruption after dehydration. B then lost alkalis as well, and precipitated groundmass An₉₃.

Terrestrial basic magmas of the calc-alkaline type, having "normal" Na/Ca ratios much higher than those of 14310, precipitate very calcic feldspars (anorthite) in enclosed magma chambers at elevated water vapour pressures, yet after eruption and water loss by volatilization these magmas precipitate much more sodic feldspars (labradorite/bytownite)^{14,15}. A specific example is provided by the basalt cinders from the Mansion pyroclastics of St Kitts¹⁵ which crystallized early plagioclase An₇₀ with olivine of Fo₇₇ after eruption, but are associated with plutonic accumulate blocks containing feldspar An₉₀₋₉₃ and olivine Fo₇₇. The compositions of these basalt cinders, and of 14310 after addition of appropriate alkalis, oxygen and some water, are compared in Table 1.

14310 was derived from a calc-alkaline basaltic magma similar in major element chemistry to a common terrestrial type (the minor element chemistry precludes any closer analogy). That magma had "normal alkali contents and was crystallizing at about 1,150° C, and 250 bars pressure in a magma chamber buried approximately 5 km below the lunar surface⁷. This wet magma, containing 1–2% H₂O, was precipitating spinel-troctolite. Some of the magma was erupted, carrying with it a few of the low density equilibrium plagioclase phenocrysts (An₉₅) from the magma chamber. At the surface the water was lost by volatilization; in the absence of dissolved water the liquidus and solidus temperatures increased rapidly, and the remaining silicate liquid found itself markedly supercooled under the new conditions. Plagioclase crystallized rapidly on the existing nuclei at first and feldspar had a sodic composition, An₇₁, reflecting the still high alkali contents of the silicate magma on eruption. Following and accompanying the water loss, however, selective volatilization of alkalis took place, again contributing towards higher liquidus and solidus temperatures. By the time general nucleation of groundmass plagioclase had occurred, the stable composition formed from the alkali depleted silicate liquid was An₉₃¹ (perhaps not significantly different from the experimentally observed An₉₂²).

The temperature of the magma, 1,150° C, was below that at which olivine or aluminous spinel are present in the dry low pressure equilibrium crystallization histories⁷; consequently neither formed during the groundmass crystallization of 14310, but relatively iron-oxide rich calcium-poor pyroxene precipitated. Oxygen loss, however, continued beyond this stage so that the final composition of 14310 was too depleted in oxygen to yield such iron-oxide rich pyroxenes in experiments, a more magnesian pyroxene and metallic iron being observed instead.

The ultimate source of the 14310 magma was massive partial melting of a water-rich lunar interior, yielding a liquid of gabbroic-anorthosite type which fractionated extensively by plagioclase crystallization close to the lunar surface. The pre-eruption 14310 magma was a residual liquid from that fractionation process⁷.

Summarizing, the petrography of sample 14310 provides the strongest evidence yet for the real operation of a process which was always a common sense probability, namely the significant alteration of magma compositions by the selective volatilization of water, alkalis and oxygen from silicate liquids erupted at 1,100–1,300° C into a vacuum of harder than 10⁻¹⁰ torr on the lunar surface. In the light of this there is no need to appeal to a special and separate accretion of the lunar crust with its

present composition, and no case for ascribing the dry, reduced and alkali-poor of lunar surface samples to the Moon as a whole.

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Received September 5, 1972.

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Refrigeration of Lunar Samples destined for Thermoluminescence Studies

I wish to provide quantitative data supporting the need for refrigeration of lunar samples, retrieved by both manned and unmanned missions, which are meant for thermoluminescence (TL) investigations. The same arguments apply to freshly fallen meteorites. This need for low-temperature storage does not seem to be generally appreciated, with the result that a good deal of useful information is irretrievably lost.

The main purposes of TL investigations of lunar (or meteoritic) material are, first, to derive the radiation and temperature histories of the samples and, second, to estimate the (cosmic-ray exposure) age of the samples or their parent bodies. In essence,

Table 1 Half-lives of First Peaks in Typical Lunar Samples held at Different Storage Temperatures

Sample (and peak temperature)	Trap parameters		Half-life τ_1 at			
	Depth E (eV)	Freq. factor s (s ⁻¹)	+20° C	-20° C	-80° C*	-196° C†
12070,112 (fines) (160° C)	1.15	$\sim 7 \times 10^{12}$	64 days	200 yr	3×10^9 yr	3×10^{54} yr
12051,15 (rock chip) (175° C)	1.05	$\sim 2 \times 10^{11}$	40 days	90 yr	3×10^8 yr	4×10^{49} yr
15261,70 (fines)‡ (140° C)	1.00	$\sim 5 \times 10^{11}$	2 days	4 yr	4×10^6 yr	1×10^{46} yr

* CO₂ snow ("dry ice") temperature.

† Liquid nitrogen temperature.

‡ This sample, which comes from the bottom of a trench ~20 cm deep on Hadley Delta, is estimated by us³ to have remained at ~250 K for the last 15,000 yr.

the amount of "natural" TL observed in a body is the result of two competing processes: the radiative filling of "electron traps", and the thermal drainage of these traps in the natural environment of the body. The mean life $\tau(T)$ of electrons in traps of "depth" E and "frequency factor" s in a body held at an absolute temperature T is given¹ by

$$\tau(T) = s^{-1} \cdot e^{E/kT} \quad (1)$$

where k is Boltzmann's constant.

Parameters E and s for each "glow peak" in a TL readout can be determined experimentally², and the values of τ at various storage temperatures calculated from equation (1). For peaks which are observed at relatively low readout temperatures (say ~150° C), half-lives τ_1 ($=\tau \times \ln 2$) at room temperature are often quite short (a few days or weeks). Low-temperature storage as well as prompt distribution of lunar (and meteoritic) samples destined for TL studies is thus called for. The loss of low-temperature peaks is particularly unfortunate in the case of lunar core tube samples which, having remained at low ambient temperatures (~240 K) on the Moon, should have displayed substantial TL in the first natural peaks, had fading been arrested by placing them in deep freeze in the laboratory. (Ideally, refrigeration should start during the return journey of the spacecraft; but allowance can be made for a few days of fading.) The same is true of samples from manned or unmanned Moon missions collected from depths as little as ~20 cm (where effective storage temperatures are ~250 K, ref. 3) or from locations which are shaded by projecting crater walls and boulders, and also of samples from any future lunar polar missions. Similarly meteorite cores (that is, regions away from "fusion crusts") may still be at or near their outer-space temperatures⁴ (~200 K) if recovered soon after an observed fall. The advantages of refrigeration

were demonstrated⁵ in the case of the Allende meteorite, which fell on February 8, 1969.

Fig. 1 shows the situation in the case of a fines sample² from Apollo 12. The first peak (see natural TL curve *A*) had obviously been totally drained during the pre-readout storage of the sample in the laboratory (at 20° C for ~18 months).

Table 1 records the half-lives of the first peak in some typical Apollo 12 and 15 samples at a number of storage temperatures of interest, as calculated from the values of E and s measured in this laboratory.

The need for refrigerating at least a small fraction of the lunar samples (preferably at liquid nitrogen temperature), immediately upon receipt in the Lunar Receiving Laboratory, is obvious from Table 1. Depositing parts of freshly fallen meteorites in the freezing compartments (~-10° C) of even ordinary refrigerators—and preferably in "deep freeze" units (~-20° C)—in museums and laboratories is strongly recommended. Prompt distribution of samples destined for TL studies is urged in all cases.

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Received September 3; revised September 20, 1972.

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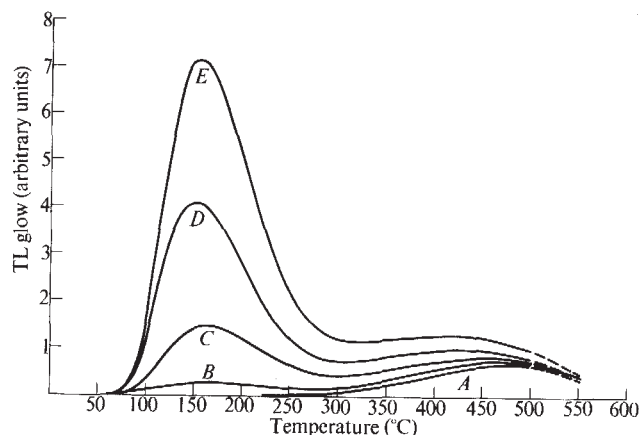


Fig. 1 Typical TL glow curves from a lunar fines sample (rate of heating, 5° C s⁻¹; black body contribution subtracted). The first peak had become drained in the natural sample (curve *A*) during its laboratory storage (for ~18 months; τ_1 ~64 days), but is reinstated upon irradiating the sample with ⁶⁰Co γ -rays. The high-temperature peak was found to have τ_1 ~ 7×10^9 yr at 20° C. *A*, Natural sample; *B*, natural sample +35 krad; *C*, +470 krad; *D*, +960 krad; *E*, +2,700 krad.

Influence of Continental Positions on Early Tertiary Climates

CONTINENTS can be positioned in their earlier places on the Cainozoic globe by means of their rock magnetism; continents for which such data are lacking can be placed in their relative positions by removing the subsequently generated seafloor. On such a reconstructed globe for a particular time in the Cainozoic the distribution of palaeoclimates, as deduced from significant rock-types and fossil plants and animals, can be plotted in their true geographical relationships. Locations of the continents and subcontinents with respect to the rotational coordinate system, as well as to each other, will largely determine the prevailing climatic trends by affecting the circulation patterns in oceans and atmosphere. Here we attempt to model, in a semi-quantitative way, the climates of the Earth in the later half of the Eocene (40–48 m.y. ago) and in the early half of the Oligocene (30–37 m.y. ago), an interval of marked climatic change.

For deciphering global palaeoclimates a reconstruction must be used which positions continents with respect to the poles

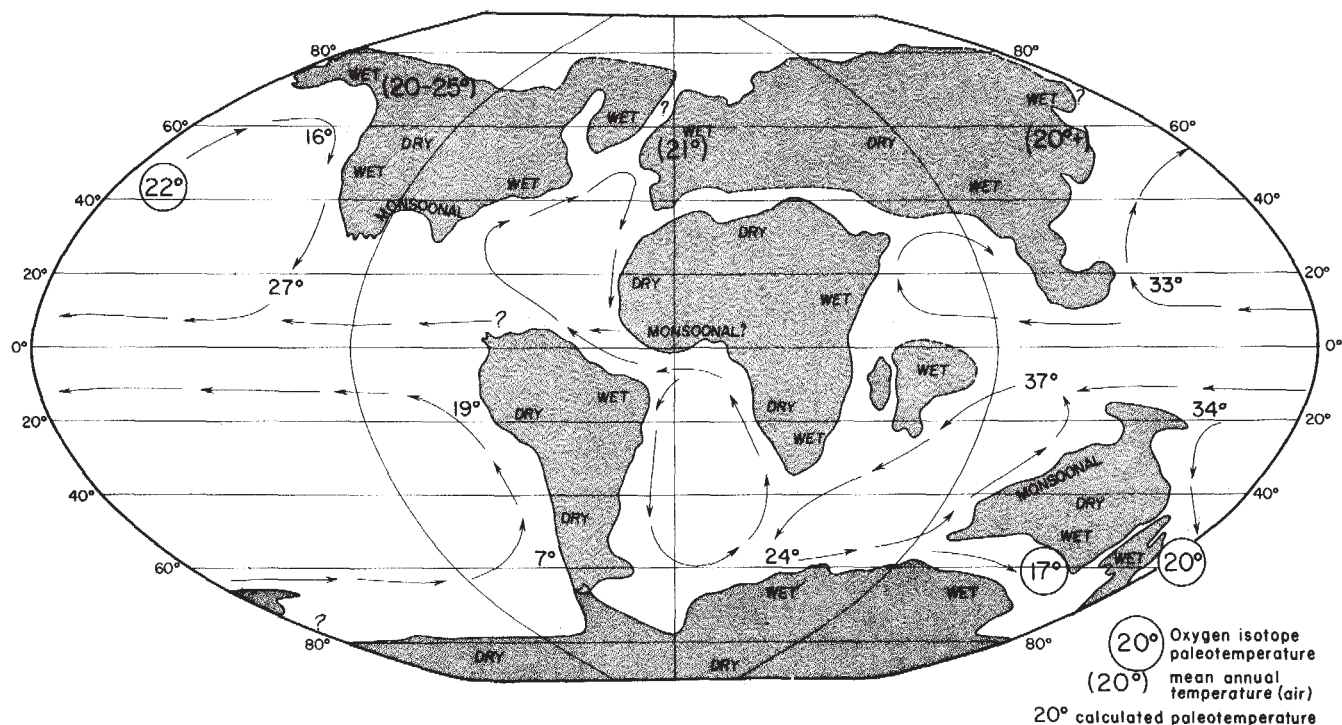


Fig. 1 Reconstruction of weather patterns in Eocene.

of the Earth's rotation, because of the strong latitude-dependence of climate. The mean palaeomagnetic pole probably approximated the position of the Earth's rotational pole throughout the Cainozoic, and deviation of the palaeomagnetic pole from the geographic pole can be treated by sampling rock intervals representing more than 10^4 yr (ref. 1). The rock magnetism data selected for use in this study consist of many samples from multiple sites, display relatively small values for α (radius of the circle of 95% confidence), and have all been a.c. magnetically cleaned. Most continents, however, can only be positioned within about 5 arc deg and the pole positions, and hence palaeolatitudinal positioning of the continents, represent a time interval of as much as 7–8 m.y., although the data were selected for geological synchronicity. Neither the geometrical nor the chronological accuracy is thus as good as in reconstructions based solely on seafloor spreading data². For palaeoclimates the advantage gained in the use of rock magnetism data first, but with reliance on seafloor data as well, lies in the greater assurance that continents are primarily positioned with respect to the pole. Moreover, no assumptions are made as to the fixity of any continent, plate, oceanic ridge or pole.

In the Eocene reconstruction (Fig. 1), positioning of most continents is based on rock magnetism—as summarized by McElhinney and Wellman³ for Australia (New South Wales shield volcanics) and Africa (Ethiopian volcanics), and by Creer⁴ for western Europe (Antrim basalts), North America (Green River Formation), and Pakistan–India (post-Deccan Traps⁵).

The positions of South America relative to Africa⁶, and of Antarctica relative to Australia–New Zealand–New Guinea^{7–9}, are determined by reversing the seafloor spreading process to its late Eocene configuration. Seafloor data are used here because of the lack of Cainozoic rock magnetism information from South America and Antarctica. The result is a much different Earth—a very wide Indopacific Ocean (spanning about 210° at the equator), a narrow Atlantic, a south polar landmass probably connecting to South America and separated from Australia by a narrow seaway, and Pakistan–India astride the equator. By the middle of the Oligocene (Fig. 2) the equatorial Pacific had been narrowed substantially, concurrently with the widening of the Australia–Antarctic seaway⁵, as a

result of the northward motion of Australia–New Guinea¹. Also, the South America–Antarctica join appears to have been interrupted by formation of the Scotia arc, though this conclusion must be tentative because the positions for Antarctica are based on seafloor spreading data alone. But this timing for formation of the arc is in accord with the post-Mesozoic, pre-20 m.y. date suggested by Dalziel and Elliot¹⁰.

Having made reconstructions for these Cainozoic intervals, we can deduce rough circulation patterns of surface currents in the world ocean, and, with less assurance, suggest resulting trends in global climates. In the late Eocene South Pacific, much of the equatorial segment of the enormous counter-clockwise gyral swept westward from Peru into the area of the present Indian Ocean, before being diverted southward by Pakistan–India toward Antarctica and eventually Australia. The north equatorial current spanned at least 150° arc deg and was deflected northward by south-eastern Asia, eventually to reach the Gulf of Alaska. The interruption of the circumpolar current in the Southern Ocean by the South America–Antarctica landmass, and possibly also near New Zealand by the Macquarie Rise system⁹, was also significant. The broad patterns of oceanic circulation are clear enough, except for the extent of exchange between the Atlantic and bordering water masses; we have not attempted to decipher detail, nor to include epicontinental seas.

Temperature distributions in the Eocene world ocean are poorly known but can be approximated by use of published oxygen-isotope determinations^{11–13} as base values. From surface-temperature maps¹⁴ we calculate warming rates for the equatorial currents (at present averaging about 0.1° C/deg longitude). Similarly, warming and cooling rates in meridional currents are about $+0.29^\circ$ C/deg latitude. These rates were applied to yield the temperature value for the Indopacific shown on Fig. 1. Mean annual temperatures for continental areas are from published analyses of fossil flora^{15–17}.

Circulation patterns during the early Oligocene differ in that the Indopacific is segmented, for the first time, by the northward motion of Australia–New Guinea. The Pacific consequently approximates its present equatorial width. Further, the circumpolar current encircles the globe in the Southern Ocean, and exchange between the Atlantic and the neighbouring Arctic Ocean is probably greater than in the Eocene. Sea

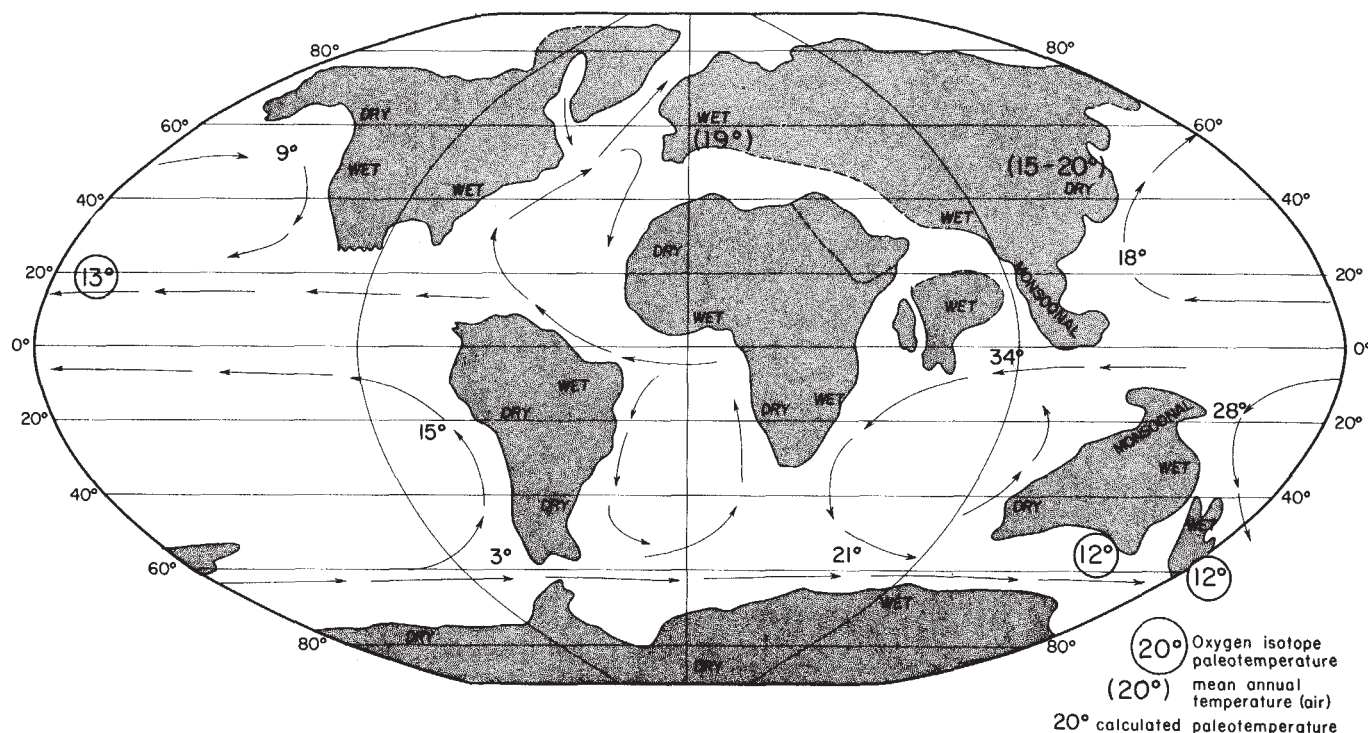


Fig. 2 Mid-Oligocene weather patterns.

surface temperatures have been estimated as before. It is clear that surface waters were colder than in the Eocene interval; they tend to be intermediate between the very warm temperatures of the Eocene and those of the present Pacific. Similarly, the few mean annual air temperatures for the Oligocene fall between present and Eocene values. This coincidence of meridional displacement of the continents with the evolution of colder global climates during the Cainozoic may be a cause and effect relationship.

The temperature distributions we deduce for the Palaeogene oceans suggest related distributions of evaporation and precipitation over the landmasses. Evaporation is best developed where warm segments of the oceans are in contact with relatively cold air, and precipitation occurs most readily where moist air is forced upward by convergence of surface air-masses or by the existence of orographic barriers. At present, precipitation is most abundant in the zones between the equator and 10° N, and near the frontal convergence zones between 30° and 50°. Precipitation is also concentrated along the west coasts of continents at 40° to 60° and on the east coasts at 15° to 30°. In the Eocene, abundant precipitation characterized the entire western margin of the Indopacific, and the wet belts probably extended to considerably higher latitudes than at present because surface waters in the oceans were warmer. Continental regions adjacent to and downwind from high surface-water temperatures in Fig. 1 are accordingly marked "wet". Similarly, areas bordering on and upwind from cold currents are considered to be "dry", as compared with the average conditions for that latitude. In these deductions we follow Lamb¹⁸, who suggests that on an ice-free globe the trade wind belts would be expanded poleward at the expense of the high-latitude westerlies. Monsoonal conditions must have existed on coastal regions facing the equator and within the range of the seasonally varying intertropical convergence zone. Important warming currents such as the Gulf Stream and the Kuro Shio were already in existence.

This suggestion of warm, wet climates at high latitudes in the Palaeogene has been well documented by the distribution of fossil plants and animals and by climatically significant rock types¹⁷. Fossil plants from the Early Eocene London Clay are largely comparable, on a taxonomic basis, with those of the modern Indo-Malayan region²⁰, although Daley²¹ has recently

cautioned against interpretation of this fossil flora in strictly tropical terms. The small admixture of temperate elements in the flora may reflect something of the seasonal variation to be expected at relatively high latitudes. Eocene floras from the Gulf of Alaska region have been designated "subtropical or paratropical", primarily on the basis of leaf morphology^{15,27}; floras similarly suggestive of warm humid climatic regimes are known from the Eocene of Hokkaido. In the southern hemisphere, palynological evidence suggests floras of "subtropical" aspect from New Zealand^{23,24} and temperate forests in the Ross Sea region of Antarctica²⁵. Other indicators of high-latitude warmth include reef corals in northern California, South Australia and New Zealand, and "lateritic" or bauxitic weathering profiles which form today in Köppen's "A" type climates of the tropics and wet subtropics. The latter soils are known from Eocene high mid-latitudes in Northern Ireland, the north-western United States, New Zealand, and northern Australia²⁶⁻²⁸. Abundant high-latitude precipitation is further suggested by widespread coals of Eocene age.

An important climatic effect of the cooling of Oligocene surface waters was to restrict the high-latitude wet belts to latitudes below 60°, although atmospheric temperatures also dropped. Monsoonal regions of continents were probably restricted as well, primarily because of the motion of the continents out of the appropriate positions.

On the basis of the geological record available, the Oligocene climatic picture appears slightly cooler and drier. Evidence from fossil floras suggests a temperature decline that was dramatic in some cases^{15,22}, gradual in others^{16,17,23,29,31}. But reef corals were able to thrive to at least 40° latitude, about 10° beyond their present range³².

Great warmth and high precipitation in the Eocene high-latitude regions can be explained at least in part by the global temperature distributions derived from calculations based on oxygen isotope data for the oceans. Because of their prolonged residence time in equatorial regions, surface waters reaching the Antarctica-Australia and Gulf of Alaska regions were much warmer than at present—their main effect would be to warm the coastal atmosphere through the processes of evaporation (transfer of latent heat from ocean to atmosphere) and subsequent condensation (conversion of latent heat to specific heat within the atmosphere). The very warm and wet conditions

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which apparently characterized latitudes beyond 45° in the late Eocene were altered during the first half of the Oligocene to conditions more like the present. Simultaneously, equatorial transport in the oceanic gyres of the Pacific was restricted, giving colder surface waters around the western Pacific and thus less efficient poleward heat transport. Colder Oligocene surface waters in high latitudes led to less evaporation and therefore to decreased precipitation and colder air temperatures. The process of poleward heat transport by the oceans operates even less effectively today, and the consequences are seen in the still lower temperatures and precipitation rates.

Feedback mechanisms arising from the existence of warm oceans may have contributed to more efficient atmospheric transfer of heat energy away from the equatorial heat source as well, but the relative significance of these processes is difficult to quantify. Today, 20% of the total poleward heat transfer is effected by ocean currents; in the Eocene, certainly, a greater proportion would have been required. We emphasize the significance of the oceans to store and transport heat, particularly if given the proper configurations of land and sea, but this is with the recognition that other direct and indirect factors undoubtedly played a part in influencing climate³³.

This study was supported by the US National Science Foundation.

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Received June 13, 1972.

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SYNTHESIS of proteins from the mRNA in eukaryotic cells differs from prokaryotes in two important ways. In the mammalian cell, the translation of mRNA in the cytoplasm is remote from the transcription in the nucleus, whereas in bacteria these two processes occur almost simultaneously^{1,2}. Bacterial protein synthesis ceases within several minutes of inhibition of RNA transcription with actinomycin³; hence, the mRNA is short-lived. In metazoan eukaryotic cells, a similar experiment indicates that protein synthesis is not immediately affected by inhibition of transcription. Thus the mRNA is much more stable—of the order of several hours⁴. In a few specialized systems, the lifetime of mRNA can be deduced by observing the decay of protein synthesis after RNA synthesis has ceased. The messenger for haemoglobin in the reticulocyte⁵ and for silk fibroin in the silkworm⁶ are examples of stable messenger molecules with a lifetime of several days. The measurement of messenger lifetime in cells with active RNA metabolism is more difficult. In the experimental approach mentioned above, new RNA synthesis was blocked with actinomycin and the subsequent decay of protein synthesis measured. Under these conditions, protein synthesis generally decreased with a 2.5 to 3 h half-life in a wide variety of systems^{4,7-9}. Interpreting the decay of protein synthesis as due to a concomitant degradation of mRNA assumes that the availability of messenger molecules is limiting in protein synthesis. Here, we shall show that mRNA decay is not

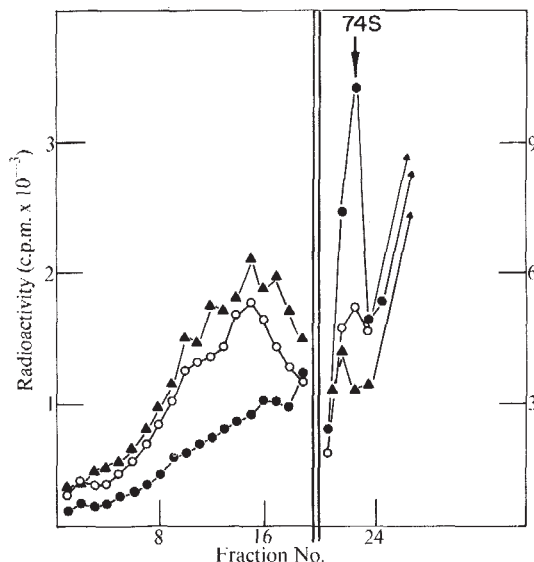


Fig. 1 The decay and rebuilding of polysomes in the presence of actinomycin. HeLa cells were grown in suspension culture as previously described²⁷. 100 ml. culture was labelled for 15 h with 2 mCi of ¹⁴C-uridine (Schwarz Biochemical, 35 mCi/mM), so that most of the label was in rRNA. Polysomes were allowed to decay in the presence of actinomycin (4 µg/ml., 3 h) and then 1 µg/ml. of cycloheximide was added to 1/3 the incubation mixture of HeLa cells (50 ml. at 4 × 10⁵ cells/ml.) for 30 additional min. For control cells, the actinomycin was omitted. Cells were harvested by centrifugation at 2,000 r.p.m. for 2 min and resuspended in RSB buffer (0.01 M NaCl, 0.01 M Tris-HCl, pH 7.4, 0.0015 M MgCl₂). The cells were then broken with 'NP40' (Shell Oil Company, 0.5%). Nuclei were removed by centrifugation at 2,000 r.p.m. for 2 min and the cytoplasm layered on an RSB sucrose (15%–30% w/v) gradient (12 ml.) and centrifuged at 40,000 r.p.m. in a 'Spinco SW 40' rotor at 4° C for 1 h. Fractions were collected directly into vials and scintillation fluid (omnifluor, methoxy ethanol, toluene) added. ▲, Polysomes from normal cells; ●, polysomes from actinomycin treated cells; ○, subsequent cycloheximide treatment. Monomers (74S) are on the right-hand scale, polysomes on the left-hand scale.

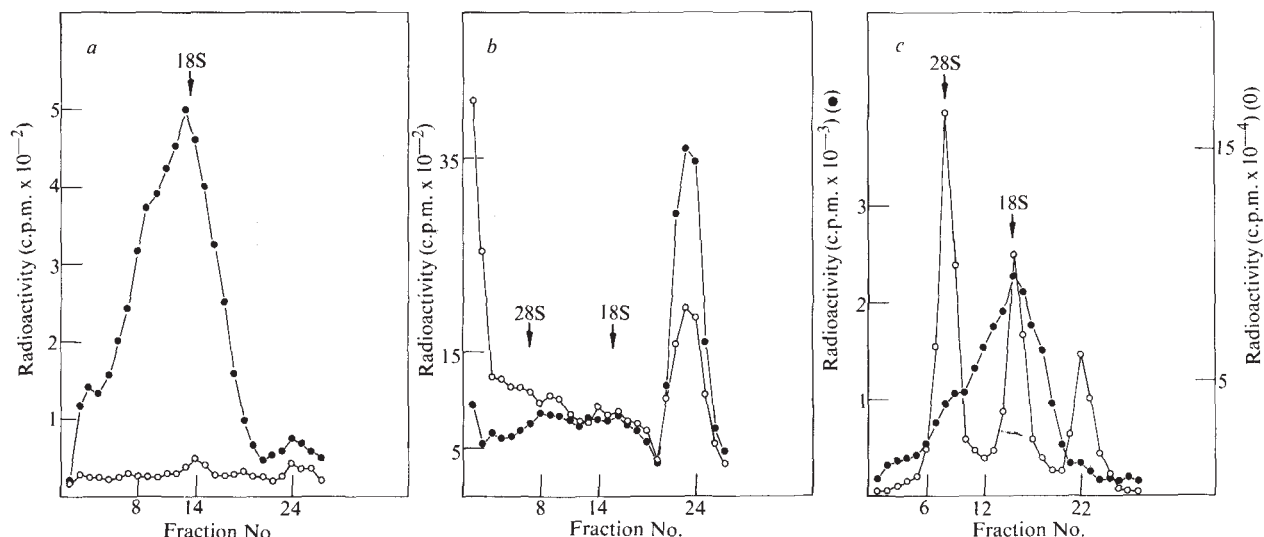


Fig. 2 Comparison of RNA hybridized to and washed through the filters under various conditions. *a* and *b* show the effect of cordycepin on the filter binding and non-binding material. 10 ml. of cell culture (2×10^6 cells/ml.) were exposed to a low level of actinomycin (0.04 $\mu\text{g}/\text{ml.}$) and ethidium bromide (1 $\mu\text{g}/\text{ml.}$). One half had cordycepin added (20 $\mu\text{g}/\text{ml.}$). The cells were then labelled with ^3H -uridine (New England Nuclear, 20 Ci/mM) for 90 min. *a* compares filter bound material in the presence and absence of cordycepin. *b* compares the washes from these filters. *c* represents the washed and bound material from cell cytoplasm labelled for 90 min (no drugs present) in 2 $\mu\text{Ci}/\text{ml.}$ of ^3H -uridine (4×10^5 cells/ml.). For all experiments, cells swollen in RSB buffer were lysed with NP40 and the cytoplasm extracted with phenol after the method of Penman²⁶ except that the phenol and chloroform were mixed previous to the extraction. The pellet after precipitation with ethanol was resuspended in 2 ml. of SDS buffer and bound to a poly U filter. Poly U filters were made after the method of Kates¹⁶. The sample of total cytoplasm was passed through the poly U filter at about 1 ml. min⁻¹. The filters were washed with 4 ml. of SDS buffer and the material bound was eluted from the filter by passing 4 ml. of a formamide solution (50% formamide, 50% elution buffer, 0.01 M Tris, pH 7.4, 0.5% SDS) at 45°C through the filter. Carrier RNA was added to eluant (made 0.1 M NaCl) and wash. Both were precipitated with alcohol and then resuspended in 0.8 ml. SDS and layered on an SDS sucrose gradient (30%–15% w/v) and centrifuged at 25,000 r.p.m. in a 'Spinco SW27' rotor for 18 h. The gradients were collected, precipitated with 5% trichloroacetic acid and assayed to radioactivity. *a*: $\circ$, cordycepin cytoplasm; $\bullet$, control (low actinomycin) cytoplasm. *b*: $\circ$, cordycepin wash; $\bullet$, control (low actinomycin) wash. *c*: $\bullet$, eluant; $\circ$, wash.

responsible for protein synthesis decay in actinomycin. Rather, mRNA appears quite stable after the administration of actinomycin. The subsequent decay of protein synthesis appears to be due to a failure in the initiation of translation.

The decrease in sedimentation pattern of polyribosomes after several hours treatment with actinomycin does not yield the expected result assuming a decay in the amount of mRNA. Disappearance of mRNA should reduce the total amount of polyribosomes but not shift the sedimentation distribution, yet polysomes from HeLa cells treated with actinomycin sediment slower than normal cells (Fig. 1). This decrease in size implies that mRNA becomes more lightly loaded with ribosomes, possibly because the rate of ribosomal initiation decreases relative to the rate of elongation. The experiment shown in Fig. 1 supports this possibility. Cycloheximide is added to both control and actinomycin treated cells at a concentration sufficient to decrease the rate of ribosome movement on message about 5-fold^{10–12}. The slower initiation is apparently balanced by the diminished elongation rate and a significant recovery of normal sized polyribosomes occurs in the actinomycin treated cells. This resurrection of polyribosomes in cycloheximide suggests that much of the mRNA is still present in the actinomycin treated cells.

The recent discovery that nearly all mRNA contains a length of 3' terminal polyadenylic acid now allows a quantitative measurement of the amount of mRNA in the cell^{13–15}. By adapting a technique developed by Kates *et al.*¹⁶, RNA containing this poly A sequence can be completely and selectively hybridized to polyuridylic acid immobilized on a glass fibre filter. The hybrid can then be melted off the filter and analysed in a sedimentation gradient. Fig. 2 shows the selectivity and efficiency of this hybridizing filter method. The first panel compares the sedimentation profiles of filter-retained and filter-transmitted RNA from the cytoplasm of HeLa cells labelled for 60 min in a low level of actinomycin (0.04 $\mu\text{g}/\text{ml.}$) sufficient to differentially suppress synthesis of rRNA^{17–19}. The RNA hybridized and then eluted from the

filter has a characteristic sedimentation pattern of mRNA¹⁹; it is heterogeneous and extending from 8S to approximately 30S with a maximum at 18S. The transmitted RNA shown in Fig. 2*b* consists of 4S RNA and a flat distribution of heterogeneous RNA extending to greater than 60S which has been shown to be unassociated with polyribosomes and is assumed to be unrelated to mRNA¹⁹. mRNA does not appear in the cytoplasm in the presence of cordycepin²⁰, an adenosine analogue; and indeed, panel A indicates that this drug inhibits the labelling of nearly all filter binding material but has little effect on transmitted RNA. Furthermore, the RNA bound to the filter is found associated with polyribosomes and is released by EDTA. It therefore appears to have the behaviour expected of mRNA. Fig. 2*c* indicates the powerful selectivity of the filters. Total cellular RNA was labelled without suppressing rRNA synthesis. The total labelled cellular RNA was bound to a filter. Although only a small percentage of this RNA is messenger, this filter technique selectively hybridizes mRNA uncontaminated by rRNA. Less than 0.2% of the input rRNA is retained on the filter.

The stability of mRNA in actinomycin was measured using poly U filters (Fig. 3). Actinomycin was added to cells after a 90 min labelling period, and measurements made at intervals several hours later. The bound RNA eluted from the filters and the transmitted RNA were analysed on sucrose gradients. The mRNA eluted from the filter is identical in its sedimentation distribution and purity in all samples. There was little decay of the mRNA eluted from the filter, even after 5 h in actinomycin. So mRNA in exponentially growing HeLa cells apparently has a half-life much longer than 6 h. It is possible, however, that actinomycin may be interfering with the normal messenger decay process, as in hormone-induced protein synthesis. We have measured the kinetics of labelling mRNA with the filter technique in the absence of any inhibitors. These experiments, to be described in another report, indicate a messenger lifetime in the order of 24 h in exponentially growing HeLa cells.

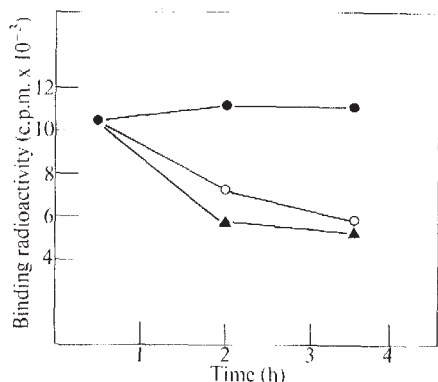


Fig. 3 Stability of messenger RNA in actinomycin and the effect of polyribosome dissociating agents. 70 ml. of cells (2×10^6 cells/ml.) was incubated for 1 h in ^3H -uridine ($10 \mu\text{Ci}/\text{ml.}$) and then actinomycin ($4 \mu\text{g}/\text{ml.}$) was introduced into the culture. 20 min after the addition of actinomycin an aliquot of cells was removed and the remaining cells were divided into 3 separate mixtures: one received $200 \mu\text{g}/\text{ml.}$ puromycin (▲), one 10 mM NaF (○), and the last was a control (●). Aliquots were removed from each mixture at 2 h and 3.5 h after the addition of actinomycin. Cytoplasm was prepared as in Fig. 2. The samples were extracted with phenol. All samples were analysed with the poly U filters after the procedure as in Fig. 2 and run on gradients along with the wash, in order to correct for variations in recovery. Total counts bound to the filter are plotted.

Mechanisms do exist in the cell which can break down mRNA relatively rapidly. In the presence of puromycin ($200 \mu\text{g}/\text{ml.}$) or fluoride ion (10 mM), the messenger can be seen to decay significantly using the poly U filter technique (Fig. 3). Similar experiments done with total cytoplasmic RNA prelabelled in low actinomycin to suppress rRNA and not bound to poly U filters yielded the same result. Thus the loss of filter binding RNA does not result from the selective and exclusive loss of the poly A sequence from the mRNA. Because both puromycin²¹ and fluoride²² are polyribosome disaggregating agents, the data suggest that mRNA stability requires intact polyribosomes and that interruption of normal translation results in rapid messenger decay. Both drugs, however, can cause serious side reactions so that this conclusion must be tentative. Our experiments indicate that mRNA appears essentially stable in the presence of actinomycin. We show also that actinomycin has the additional effect of slowing the initiation of ribosomes onto the mRNA. Therefore, the rate of protein synthesis is not a measure of the availability of mRNA. The results using polyribosomal disruptors imply that messenger must be on polyribosomes in order to be stable. Although the demonstration of the existence of mRNA in the cytoplasm does not prove it functional, the fact that it can be used to build up polyribosomes for as long as 3 h after actinomycin treatment suggests that it probably remained functional for at least this long. A report in preparation indicates that after a 24 h chase, labelled mRNA is still associated with polyribosomes.

In some differentiated systems^{23,24}, some protein synthesis has been shown to be unaffected by actinomycin, possibly indicating a different initiation mechanism. Moreover, the apparent long lifetime of mRNA in actinomycin may explain a puzzling result. McAuslan showed in 1963 that actinomycin prevented the shutoff of early enzyme synthesis when administered 3 h after vaccinia virus infection²⁵. Remarkably, synthesis of thymidine kinase continued linearly for 16 h. Should initiation of translation in vaccinia be unaffected by actinomycin, the long lifetime of the early vaccinia message could be understood in the light of our results. The apparent long life of mRNA in HeLa cells suggests that equating protein synthesis decay with messenger lifetime may often be incorrect. In particular, rapid changes in the rate of production of particular products may, in part, be controlled by mechanisms involving translational control.

This work was supported by the National Institutes of

Health and the National Science Foundation. S. P. is a career development awardee of the US Public Health Service, R. H. S. is a recipient of a Damon Runyon fellowship.

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High Stability of Messenger RNA in Growing Cultured Cells

THE discovery that messenger RNA (mRNA) in mammalian cells contains sequences of polyadenylic acid (poly A) about 200 nucleotides long localized at the 3'-OH terminus of the mRNA molecule¹⁻⁵ has raised questions about the function of poly A, but has also provided a powerful new tool for the study of mRNA metabolism. I have measured the stability of mRNA in exponentially growing mouse L-cells without having to resort to the use of inhibitors of RNA synthesis, and found that poly A-containing mRNA has a half-life of 10 h, and thus turns over approximately once per cell generation (15 h). This high stability is in striking contrast to the 3-4 h half-life for mRNA in cultured cells obtained by following the decay of polyribosomes after treatment with a high dose of actinomycin D^{6,7}. This, however, agrees with the finding of Cheevers and Sheinin⁸ that there is a long-lived fraction of mRNA in cells treated with a low dose of actinomycin D. Singer and Penman⁹ have simultaneously found that poly A-containing mRNA in HeLa cells turns over about once per cell generation.

The method I have used for measuring mRNA stability is based on the kinetics of approach to steady state labelling of polyribosomal RNA adsorbed to 'Millipore' filters in conditions specific for binding of poly A². The equation used is

$$A/A_{\infty} = 1 - e^{-\ln 2 (1/T_D + 1/T_{1/2})t} \quad (1)$$

where A is the specific activity of a cell component at time t , A_{∞} is the specific activity as $t \rightarrow \infty$, T_D is the cell doubling time, and $T_{1/2}$ is the half-life of the cell component. The derivation of equation (1) is discussed at the end of the text.

Determining the mRNA half-life from equation (1) requires that the cells are in exponential growth during the course of labelling, and also that the RNA is labelled from a precursor pool of constant specific activity. To ensure that these conditions were satisfied, cells were labelled with ³H-uridine in standard growth medium supplemented with the minimum amount of unlabelled uridine ascertained by Rake and Graham¹⁰ to be sufficient for steady state incorporation into mouse L-cells.

To determine T_D and to verify that the cells were growing exponentially, cell counts were made periodically during the course of labelling. Fig. 1 shows that the cells grew exponentially with $T_D = 15$ h from an initial density of 1.2×10^5 cells/ml. to a final density of 4.5×10^5 cells/ml.

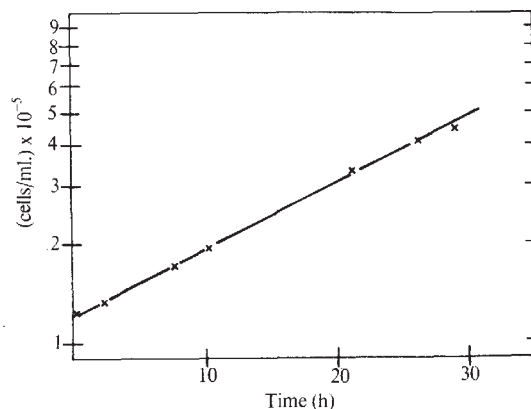


Fig. 1 Growth of cells under conditions for steady state labelling. Mouse L-cells were maintained in suspension cultures as described¹¹. An exponentially growing culture was diluted with fresh prewarmed standard growth medium to 1.2×10^5 cells/ml. Unlabelled uridine at 3 μ g/ml. and ³H-5-uridine (>20 Ci/mmol, obtained from New England Nuclear Corp., Boston, Massachusetts) at 2 μ Ci/ml. were added at zero time. At intervals cell counts were made with a Coulter electronic particle counter (Coulter Electronics, Chicago, Illinois).

To verify that the RNA was labelled from a precursor pool of essentially constant specific activity, the time course of ribosomal RNA (rRNA) labelling was followed. Since rRNA does not turn over at a measurable rate in growing cultured cells¹², then labelling of rRNA should follow equation (1) for the case where $T_D = 15$ h and $T_{1/2} \rightarrow \infty$. To express the measured specific activity of rRNA (c.p.m./ μ g) in terms of A/A_{∞} , the prediction from equation (1) that $A/A_{\infty} = 0.5$ at 15 h after the rRNA begins to appear in the polyribosomes was used. It was estimated by linear extrapolation of the early data points that rRNA begins to appear in the polyribosomes at a constant rate at one hour after the beginning of labelling. Fig. 2a shows that, allowing for a lag of one hour in the entrance of rRNA into the polyribosomes, the labelling of rRNA closely follows the curve described by equation (1) for $T_D = 15$ h and $T_{1/2} \rightarrow \infty$. It seems safe, therefore, to assume that the RNA was labelled from a precursor pool of essentially constant specific activity.

Having obtained the expected results for the kinetics of labelling of rRNA, I proceeded to estimate the $T_{1/2}$ of poly A-containing mRNA (polyribosomal RNA bound to 'Milli-

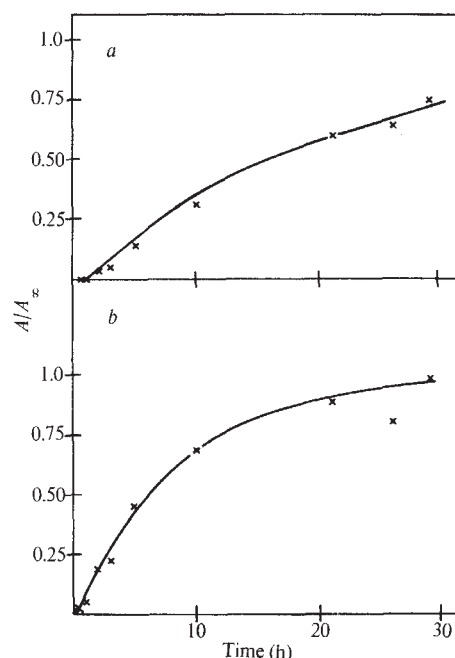


Fig. 2 a, Kinetics of labelling of rRNA. At intervals, 50 ml. aliquots of the culture described in Fig. 1 was harvested by pouring over frozen crushed physiological salt solution. Cytoplasmic extracts were prepared by lysing cells with 0.05% 'Triton X-100' (Rohm and Haas Co., Philadelphia, Pennsylvania) in isotonic buffer¹¹. The 16,000g supernatant was layered on linear 15-45% (w/w) sucrose gradients in 0.05 M KCl, 0.02 M Tris, pH 7.6, 0.002 M MgCl₂ and centrifuged 2 h at 35,000 r.p.m. in the Beckman SW40 rotor. The gradients were collected through the flow cell of a recording ultraviolet spectrophotometer. Polyribosomes (ribonucleoprotein structures sedimenting faster than 80S) were recovered by precipitation at -20° C with 2.5 volumes of ethanol and RNA was extracted with a mixture of chloroform and phenol¹³. The RNA was ethanol-precipitated, redissolved in buffer (0.1 M NaCl, 0.01 M NaCH₃COO, pH 6, 0.001 M EDTA). Aliquots were assayed for A_{260} and for acid insoluble radioactivity. Because, after extended labelling, nearly all incorporation of RNA precursors into polyribosomes is into rRNA, total trichloroacetic acid insoluble radioactivity was used as a measure of rRNA labelling. RNA insoluble in cold 5% trichloroacetic acid was collected on 'Millipore' HA filters ('Millipore' Corp., New Bedford, Massachusetts) and counted in a liquid scintillation counter. The solid line represents the theoretical curve (equation (1)) for $T_D = 15$ h, $T_{1/2} \rightarrow \infty$, with a lag of 1 h. b, Kinetics of labelling of poly A containing mRNA. Samples of the polyribosomal RNA prepared as described in Fig. 2a were diluted 50-fold with buffer (0.5 M KCl, 0.01 M Tris HCl, pH 7.6, 0.001 M MgCl₂) and filtered through 'Millipore' HA filters². Bound radioactivity, which represents poly A-containing mRNA, was assayed in a liquid scintillation counter. The solid line represents the theoretical curve (equation (1)) for $T_D = 15$ h, $T_{1/2} = 10$ h, with a lag of 0.25 h.

pore' filters under conditions specific for poly A adsorption). For this purpose, I used $T_D = 15$ h and assumed a lag of 0.25 h in the time of appearance of poly A-containing mRNA in the polyribosomes. This lag was estimated by extrapolation of the early data points, and is in agreement with a previous estimate⁵. $T_{1/2}$ was estimated by trial and error by plotting the curves described by equation (1) for various values of $T_{1/2}$ and fitting the data to these curves, using the times predicted by equation (1) for the chosen values of $T_{1/2}$ at which $A/A_{\infty} = 0.5$ to express the observed specific activity (c.p.m. bound to filter/ μ g of polyribosomal RNA) in terms of A/A_{∞} . Fig. 2b shows that a good fit was obtained with $T_{1/2} = 10$ h. With $T_{1/2} = 7$ h or $T_{1/2} = 13$ h, the data points deviated noticeably from the theoretical curves. A half-life of 10 h corresponds to a mean lifetime ($T_{1/2}/\ln 2$) of 14.4 h, which is nearly equal to T_D . It is therefore concluded that poly A-containing mRNA in exponentially growing L-cells turns over once per cell generation. Furthermore, there is no indication that poly A-containing

mRNA has multiple components which differ with respect to half-life; the data appear to be adequately explained by a single value of $T_{1/2}$.

The 10 h half-life of poly A-containing mRNA in L-cells estimated by the kinetics of approach to steady state labelling does not agree with the 3–4 h half-life of mRNA estimated from the rate of decay of polyribosomes in a high dose of actinomycin D^{6,7}. Three possible explanations for this discrepancy suggest themselves: first, poly A-containing mRNA is not representative of total mRNA; second, actinomycin D converts growing cells to non-growing cells which have an intrinsically higher rate of mRNA turnover; third, actinomycin D directly causes abnormal decay of polyribosomes. The first possibility can be eliminated, as it is known that nearly all (>90%) of mRNA in L-cells contains poly A⁵. It is not yet possible to distinguish between the other two possibilities.

The only mammalian mRNA known to lack poly A is histone mRNA^{5,14} which seems to turn over more rapidly than other types of mRNA^{7,15,16}. The absence of poly A from histone mRNA and its presence in other, longer-lived types of mRNA suggest that poly A may have a role in determining mRNA stability, and, therefore, in controlling the translation of mRNA.

Equation (1) was derived as follows: in an exponentially-growing population in which the amount per cell of a component C is constant, the amount of C at time t is given by $C = Co e^{(\ln 2/T_D)t}$ where Co is the amount of C at $t=0$, and T_D is the doubling time of the population. If C turns over stochastically, then in a labelling experiment the unlabelled molecules initially present will disappear at a rate determined by their half-life. Therefore, the amount of unlabelled material C_u present at time t is $C_u = Co e^{-(\ln 2/T_{1/2})t}$ where Co is the amount present at $t=0$ and $T_{1/2}$ is the half-life. It is assumed that all molecules synthesized at $t>0$ have the same average specific activity. The fraction of C unlabelled at time t is $C_u/C = e^{-\ln 2(t/T_D + 1/T_{1/2})}$. The fraction of C labelled at t is $1 - C_u/C$. If the radioactive precursor is drawn from a pool of constant specific activity, then $1 - C_u/C = A/A_\infty = 1 - e^{-\ln 2(t/T_D + 1/T_{1/2})}$.

This work was supported by grants from the National Science Foundation and the US Public Health Service, and an appropriation from the Commonwealth of Pennsylvania to Dr R. P. Perry. I thank Mr Gerald Sterling for technical assistance.

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Received May 10, 1972.

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Rapid Diagnosis of Scrapie in the Mouse

RESEARCH into scrapie, an enigma of veterinary medicine which may embody principles of considerable interest for human disease, commonly involves demonstration of agent activity or its titration. A great step forward was made by Chandler¹ when he showed the disease could be produced in mice, and biological titration in these animals is currently widely employed. This takes 6–8 months. From a recent study of multiple sclerosis, with which scrapie has been linked², we were led into an immunological study of scrapie itself. We found that brain or spleen from a scrapie mouse when injected into a guinea-pig (adult Hartley) led to the appearance of blood lymphocytes which were more highly sensitized to scrapie brain or spleen than to normal brain or spleen.

In early experiments, guinea-pigs were inoculated intracutaneously with 0.1 ml. of 10^{-1} suspension of scrapie brain or scrapie spleen in sterile saline. Control animals were similarly injected with brain or spleen from normal mice which had been injected some weeks previously with normal brain suspension. At intervals after 5 days, 2–3 ml. of blood was removed by cardiac puncture and lymphocyte sensitization to the scrapie and normal suspensions estimated by the sensitive and highly specific macrophage electrophoretic migration (MEM) method³. In principle the method depends on the release by sensitized lymphocyte of a protein which slows migration of normal guinea-pig macrophages in an electric field (macrophage slowing factor (MSF)—which may be identical with macrophage inhibitory factor (MIF)). Our measurements have been carried out in a Zeiss cytopherometer and full experimental details have been given⁴.

Guinea-pig blood lymphocytes were prepared by the method of Coulson and Chalmers⁵, as modified by Hughes and Caspary⁶, and normal guinea-pig macrophages by washing out the peritoneal cavity with heparinized Hanks solution 6–8 days after injection of sterile liquid paraffin. In order to obviate a two-way mixed lymphocyte reaction (at least for the duration of the test) the normal exudate was subjected to 100 rad γ -irradiation³.

10^{-1} scrapie brain, scrapie spleen, normal brain and normal spleen suspensions were used as test antigen and for later testing lymphocyte sensitization. 0.5×10^6 guinea-pig blood lymphocytes were incubated for 90 min at 20°C with 0.1 ml. of a 10^{-1} suspension of scrapie mouse brain or spleen (cleared by spinning at 1,800g for 10 min) in the presence of 10^7 irradiated normal macrophages. Control tubes comprised lymphocytes and macrophages without antigen. The migration time of macrophages in each specimen was measured by timing ten cells (readily identified by their size and paraffin droplet content) in each direction of the potential difference so that a mean (with SD) from twenty readings could be calculated. A full protocol from one specimen is given by Caspary and Field⁴. All measurements were made "blind" and results unscrambled later. If t_c = migration time of control macrophage in the absence of antigen and t_e = migration time when antigen is present; then in general $t_e > t_c$ and $t_e - t_c / t_c \times 100$ is a measure of lymphocyte sensitization to the antigen.

The scrapie brain and spleen with which the guinea-pigs were inoculated were titred out by inoculation into groups of 6 mice at dilutions of 10^{-1} through 10^{-7} . The animals were observed for eight months and all clinical scrapie diagnoses were checked histologically.

Guinea-pigs were immunized with either scrapie or normal mouse brain and spleen, 0.1 ml. of a 10^{-1} suspension being inoculated intracutaneously in the dorsum of the right foot (Table 1a, b). The guinea-pig lymphocyte sensitization to normal brain and normal spleen is always greater when the animal has been injected with scrapie material than with normal (as pointed out by Gardiner⁷ with respect to circulating antibody in rabbit experiments). Moreover, the scrapie-normal difference (when scrapie brain or spleen is used as test antigen

for the lymphocytes) is greater in the guinea-pig immunized with scrapie brain than with normal brain. The difference appears to be greatest between five and thirteen days. Table 1b shows that the same is true when guinea-pigs immunized with scrapie or normal spleen are tested for cellular sensitization to scrapie and normal brain and spleen. The results with spleen are particularly interesting since this organ whilst rich in agent ($LD_{50} = 10^{-4.7}$) shows no morphological change⁸ so that the test antigen being used might well be the scrapie agent itself (or scrapie-altered but morphologically normal membrane).

Table 1 Demonstration of Scrapie Activity in Mouse Brain and Spleen

	a, Mouse scrapie brain injected guinea-pig 10^{-1}				b, Mouse scrapie spleen injected guinea-pig 10^{-1}			
	5 days	13 days	22 days	34 days	7 days	11 days	25 days	35 days
EF	12.0	15.3	11.1	4.7	6.6	9.2	11.3	2.2
Normal brain	11.3	12.5	10.3	3.4	3.4	8.9	7.6	0.8
Scrapie brain	16.5	17.9	14.3	5.4	10.1	14.2	12.3	2.6
Brain difference	5.2	5.4	4.0	2.0	6.7	5.3	4.7	1.8
Normal spleen	5.2	6.1	6.1	1.7	11.5	12.2	10.8	1.8
Scrapie spleen	9.6	11.1	10.4	3.5	17.4	17.9	15.3	3.9
Spleen difference	4.4	5.0	4.3	1.8	5.9	5.7	4.7	2.1

	Mouse normal brain injected guinea-pig				Mouse normal spleen injected guinea-pig			
	5 days	13 days	22 days	34 days	7 days	11 days	25 days	35 days
EF	12.0	13.5	9.6	3.0	6.2	8.7	8.2	2.4
Normal brain	9.9	13.7	9.4	2.5	4.0	6.2	6.9	1.5
Scrapie brain	12.1	15.5	10.3	3.2	5.7	8.2	8.4	2.4
Brain difference	2.2	1.8	0.9	1.3	1.7	2.0	1.5	0.9
Normal spleen	4.0	5.9	6.2	1.2	13.9	13.9	11.4	2.7
Scrapie spleen	6.2	7.7	7.6	2.2	14.6	15.7	13.3	3.5
Spleen difference	2.2	1.8	1.4	1.0	0.7	1.8	1.9	0.8

Adult Hartley guinea-pigs inoculated intracutaneously in the dorsum of the right foot with 0.1 ml. 10^{-1} scrapie brain or spleen (titre 10^{-6} , $10^{-4.7}$ respectively) and lymphocyte sensitization to scrapie and normal brain measured at intervals by the macrophage electrophoresis method of Field and Caspary³. Results expressed as percentage slowing (*loc. cit.*).

In the case of inoculation with scrapie brain, however, the difference may perhaps be attributed to morphological changes (especially astrocyte increase) in the inoculated material. Having established the quantitative difference in response to scrapie as opposed to normal tissue we used this method to test out scrapie activity.

Guinea-pigs were injected with scrapie brain at dilutions of 10^{-1} to 10^{-6} and the lymphocytes examined for sensitization to scrapie brain or spleen and normal brain or spleen at six or seven days and at seventeen or eighteen days. For comparison a similar study was made in guinea-pigs injected with normal brain (Tables 2 and 3). In the scrapie brain sensitized guinea-pig the difference in lymphocyte sensitization to scrapie as compared with normal brain is 2.5% ($P < 0.01$) even at 10^{-6} original inoculum level when the guinea-pig is tested at six days but falls to 2.0% ($P = 0.1 - 0.05$) by sixteen days. When sensitization to scrapie spleen and normal spleen in these animals is compared, the difference is significant only in guinea-pigs which have received 10^{-4} scrapie brain.

Animals injected with normal brain showed no significant difference when their lymphocytes were tested with scrapie as opposed to normal brain or spleen (though in general the values with the former were higher).

Guinea-pigs were sensitized by injecting scrapie spleen at 10^{-1} to 10^{-6} and their lymphocytes tested for sensitization to scrapie brain and spleen (Table 3). A significant difference between sensitization to scrapie and normal spleen still exists at 10^{-5} ($P < 0.01$) with spleen as antigen but barely with brain (2.1% difference; $P = 0.05$), showing apparent scrapie antigenicity in the 10^{-5} dilution.

The titre of the scrapie brain inoculated into the test guinea-pigs was calculated to be $LD_{50} = 10^{-5.6}$. We noted that

Table 2 Titration of Scrapie Activity from Mouse Brain

Antigen	Lymphocytes tested at 6 days					Lymphocytes tested at 16 days		
	10^{-1}	10^{-2}	10^{-3}	10^{-4}	10^{-6}	10^{-2}	10^{-4}	10^{-6}
% macrophage slowing								
Scrapie brain								
Normal mouse brain	11.3	11.0	8.1	5.7	3.7	11.1	5.9	3.9
Scrapie brain	16.5	14.1	11.7	9.6	5.2	14.7	9.9	5.9
Brain difference	5.2	3.1	3.6	3.9	2.5	3.6	4.0	2.0
Normal mouse spleen	5.2	6.1	4.7	3.9	1.7	5.9	4.7	2.9
Scrapie mouse spleen	9.6	10.8	8.1	7.4	3.2	10.1	8.1	4.4
Spleen difference	4.4	4.7	3.4	3.5	1.5	4.2	3.4	1.5
Normal brain								
Normal mouse brain	8.9		7.1	5.9	9.6	5.5	4.4	
Scrapie mouse brain	10.3		7.9	6.7	10.1	6.5	5.2	
Brain difference	1.4		0.6	0.8	0.5	1.0	0.8	
Normal mouse spleen	5.4		2.5	3.0	6.1	4.4	3.5	
Scrapie mouse spleen	6.2		3.8	4.2	7.2	5.7	4.5	
Spleen difference	0.8		1.3	1.2	1.1	1.3	1.0	

Guinea-pigs inoculated with scrapie mouse brain with a titre of $10^{-5.6}$. 0.1 ml. inoculated at different dilutions.

Table 3 Titration of Scrapie Activity from Mouse Spleen

Antigen	Lymphocytes tested at 7 days						Lymphocytes tested at 17 days		
	10^{-1}	10^{-2}	10^{-3}	10^{-4}	10^{-5}	10^{-6}	10^{-2}	10^{-4}	10^{-6}
% macrophage slowing									
Scrapie spleen									
Normal mouse brain	3.4	4.5	2.7	1.0	1.7	0	4.7	1.5	0.7
Scrapie brain	10.1	10.3	7.2	4.4	3.8	1.2	10.1	4.9	1.5
Brain difference	6.7	5.8	4.5	3.4	2.1	1.2	5.4	3.4	0.8
Normal mouse spleen	11.5	12.6	10.4	10.6	8.1	4.5	11.1	10.0	4.4
Scrapie mouse spleen	17.4	17.0	15.3	14.4	10.8	6.3	15.0	13.8	6.2
Spleen difference	5.9	4.4	4.9	3.8	2.7	1.8	3.9	3.8	1.8
Normal spleen									
Normal mouse brain	4.5		1.5		1.0	6.4	3.0	1.8	
Scrapie mouse brain	5.9		2.9		1.2	7.6	4.4	2.9	
Brain difference	1.4		1.4		0.2	1.2	1.4	1.1	
Normal mouse spleen	11.1		9.1		4.7	10.5	7.9	4.7	
Scrapie mouse spleen	12.4		10.2		6.4	11.5	8.9	5.6	
Spleen difference	1.3		1.1		1.7	0.9	1.0	0.9	

Guinea-pigs inoculated with spleen suspension with a titre of 10^6 . 0.1 ml. inoculated at different dilutions.

1/6 mice developed scrapie at 10^{-6} and 1/6 at 10^{-7} and the immunological test suggests that some activity is still present at 10^{-6} . The titre of spleen used for immunizing the guinea-pigs was $LD_{50} = 10^{-5}$. The results of *in vivo* titration therefore agree with the *in vitro* immunological assay, and the significance of these results is two-fold. (1) Attempts to show the existence of circulating antibody and/or specific antigen in scrapie disease have not been successful^{7,8}. Gardiner did show by inoculation of rabbits with scrapie and non-scrapie spleen material that the latter had increased antigenicity *in vivo*. The present work extends these very important findings in that it suggests that this is true also of lymphocyte sensitization and that the reactivity is greater against scrapie material (both brain and spleen) than against normal material. This suggests that some specific antigen—perhaps the agent itself, or specifically agent-altered material since the spleen is so potent as a testing tissue—may be active. The macrophage electrophoretic migration (MEM) method is exquisitely responsive to minor changes in antigenic determinant structure and appears to be able to distinguish between normal and scrapie tissue. It is, of course, not possible to decide whether the difference resides in the presence of a specific scrapie agent or whether we are dealing

with a structural (antigenic) change induced by the agent. However, if scrapie infected brain is split into a neuronal and glial compartment by the method of Giorgi (unpublished) then, contrary to expectations based on the precocious glial hypertrophy, higher scrapie titre is associated with the neuronal rather than the glial fraction and it is in the former that particles with the size and structural characters postulated for scrapie agent have been found⁹. (2) Lymphocyte sensitization test enables the presence of scrapie to be established and titration carried out within 8 days, an important saving in time during the study of this fascinating condition. Further experiments are in progress involving the use of Freund's adjuvant to boost the responses. A full account of these experiments will be published elsewhere.

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Received July 25, 1972.

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Scrapie Agent and Neurones

THE nature of the scrapie agent^{1,2} is of considerable interest especially as to whether it is a virus in the strict sense, or some replicating agent devoid of nucleic acid^{3,4}, or indeed whether scrapie disease represents a transmissible progressive biochemical transformation of cellular membranes^{5,6}. Thorough electron microscope studies have been carried out in the disease and, although a number of particles have (not unexpectedly) been described^{7,8}, their role in scrapie is unconfirmed, especially as none has the special features associated with virions or has the size or other characters currently attributed to the agent.

During careful search in the brain of the scrapie rat granular inclusion bodies were seen in the cytoplasm of apparently normal neurones and their axons in the sites studied; the cortex, Ammon's horn and thalamus. At high magnification these uninteresting neuronal inclusions comprised very large numbers of minute elongated particles about 60 nm long and 20 nm wide, with a dense linear core about 4 nm wide (Figs. 1 and 2). When these particles had once been recognized they were also quickly found in blocks of scrapie rat brain which had been examined years ago.

A thorough and realistic search of comparable normal rat brain has not revealed similar particles though the vagaries of electron microscope sampling must constantly be borne in mind. The size of the particles is about that deduced from modern infectivity filtration experiments⁹. Moreover the size of central rod accords well with that of the target nucleic acid estimate made by Alper *et al.*^{3,4} and by Latarjet *et al.*¹⁰.

If the particles described are indeed scrapie agent, it is surprising to find them in neurones rather than glial cells since there is widespread agreement that the first element in the nervous system to react in the scrapie process is the astroglia¹¹⁻¹³ and the working assumption has been made that colonization of astroglial cells by the agent is responsible for their precocious hypertrophy. Indeed we have tended to

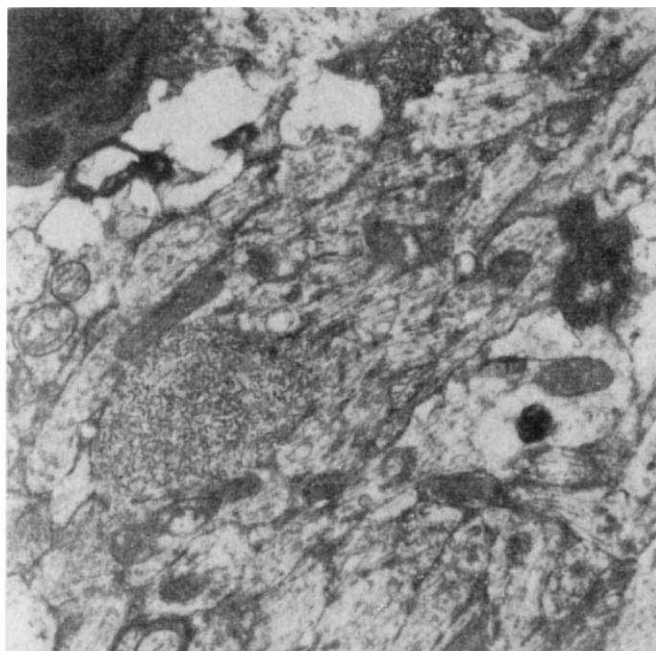


Fig. 1 Scrapie rat thalamus. Axon close to a nerve cell body showing a colony-like accumulation of small elongated particles not bounded by membrane. The neurotubular and other constituents of the axon are normal. $\times 20,625$.

concentrate search on glial cells, even though unusual neurological reactions may be detected early in the incubation period¹⁴. Simultaneously with the electron microscope findings here reported, unexpected collateral evidence has emerged that the agent may in fact proliferate in neurones rather than glial cells.

Table 1 Immunological Titration of Scrapie Activity

Antigen	Neuronal compartment				Glial compartment			
	10 ⁻¹	10 ⁻²	10 ⁻³	10 ⁻⁴	10 ⁻¹	10 ⁻²	10 ⁻³	10 ⁻⁴
Normal mouse brain	9.1	9.9	8.5	6.7	8.6	7.7	7.1	6.9
Scrapie mouse brain	14.6	13.9	11.3	9.2	11.3	8.1	8.4	8.1
Brain difference	5.5	4.0	2.8	2.5*	2.7	0.4	1.3	1.2
Normal mouse spleen	6.0	5.9	5.7	4.9	6.2	6.2	5.6	4.4
Scrapie mouse spleen	9.7	9.4	9.1	8.2	9.1	8.2	6.9	6.5
Spleen difference	3.7	3.5	3.4	3.3	2.9	2.0	1.3	2.1

* With the degree of scatter in our readings a difference of 2.5% corresponds to $P < 0.01$.

Guinea-pigs inoculated intracutaneously in right foot with 10⁻¹ to 10⁻⁴ preparations of neurones and glial cells from mouse scrapie brain (0.1 ml. in each case; equivalent to 300 μ g to 0.3 μ g protein). Sensitization of the guinea-pig lymphocytes with respect to 10⁻¹ scrapie brain and spleen and also to normal brain and spleen is measured by the macrophage electrophoretic slowing method and expressed as percentage slowing of macrophages¹⁷.

Preparations of neuronal and glial perikarya have been obtained from scrapie mouse brain using a modification (Giorgi, unpublished work) of the method of Sellinger *et al.*¹⁵. To these preparations we have applied the macrophage electrophoretic method of titration of scrapie agent¹⁶ in a study of the apportionment of scrapie activity in infected brain as between neuronal and glial compartments. The method briefly consists in immunizing guinea-pigs with successive dilutions of the material under test and measuring the degree of sensitization developed by the lymphocytes to scrapie brain (or spleen) and to normal brain (or spleen) as antigen. To our surprise there was clearly more scrapie activity associated with the neurone enriched fraction than with the glial compartment (Table 1). It might well be that the scrapie agent (or process) affects, or at least reaches greater development within, neurones

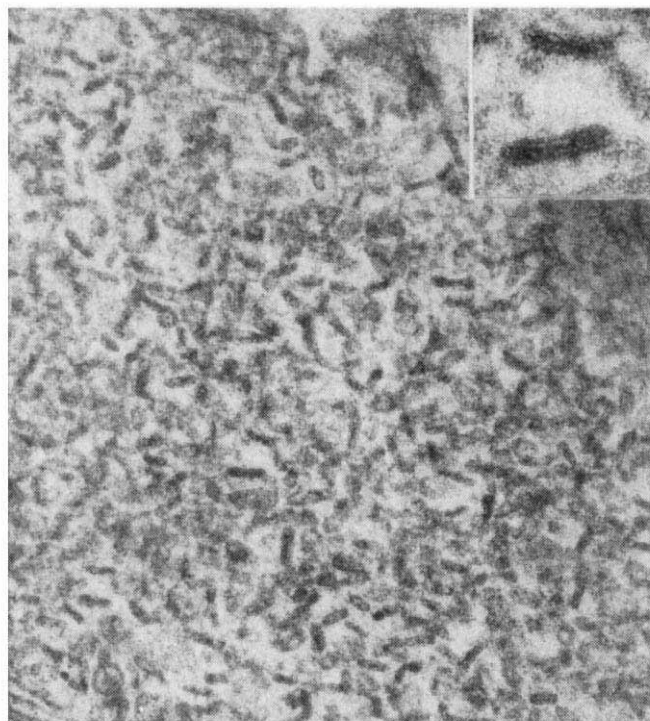


Fig. 2 High power view of colony. Note elongated particles often with dense core (see inset), about 60 nm long and 20 nm wide. The core is about 4 nm thick. Tilting and rotating the preparation confirm the discrete character of the particles. $\times 72,000$; inset, $\times 208,000$.

and that glial changes, even though they are more conspicuous, have the consequential character they are usually assigned in neuropathology. The present work illustrates the synergism of different disciplines in approaching the problem of slow infection.

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Received July 25, 1972.

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Pheromone Inactivation by the Gut of *Periplaneta americana*

RECENT interest in insect pheromones has been stimulated by the notion that the use of pheromones in traps would have the merit of specificity and would reduce the need for broad-casting insecticides¹.

The sex pheromone of the female cockroach *Periplaneta americana* (L)² elicits a series of distinctive reactions from the male. Increasing concentrations of pheromone evoke responses corresponding to increasing degrees of sexual motivation³. This behaviour has been the basis of an assay which reveals that cockroach gut can reduce the activity of pheromone preparations. Gut from males and non-producing females is more effective in this respect than that from pheromone-producing females.

Periplaneta americana sex pheromone is first produced by adult virgin females 7–11 days after the final moult. Onset of production is under the control of the corpora allata (Yamamoto in ref. 4). Pheromone activity ceases when the cockroach is mated⁵, and can also be decreased by anaesthesia or cold (my unpublished work).

The source of sex pheromone in the cockroach is under debate. Bodenstein⁶ reports considerable activity in faeces and in acetone or methanol extracts of midgut and crop. Cockroach gut organ cultures on silk screen rafts⁷ on a special medium (work in preparation) maintained their structure for several weeks but produced no pheromone. The possible presence of anti-pheromone activity in the mid and hind gut was then investigated.

Fig. 1 shows the pheromone activity of preparations from non-producing females consisting of 5 ml. of saline, one section of gut everted and rinsed (except for the controls) and two drops of a crude extract of pheromone from filter papers which had been used to line beakers containing active virgin females. As assay, two drops from each preparation were placed on a strip of filter paper and held above a jar of ten male cockroaches for 1 min. The males were graded on a scale of 0 to 4 for responses corresponding to an increasing degree of sexual motivation. On this scale no response is 0; "attention" and beating of antennae, 1; searching, 2; short random dashes, 3; wing flutter and arching of abdomen, 4. Each test was performed with seven or more jars of males and the scores were summed. Because all ten cockroaches in the jar rarely showed identical responses, a level of response was assigned when shown by three or more cockroaches, the rest being inactive. When more than one level of response was shown I assigned the highest level when shown by three or more. These assays were conducted in a lighted laboratory several hours after the cockroaches had been brought from the insectary, so that I was measuring the capacity of the sex pheromone to override background stimuli.

Analysis of variance shows that scores for control and foregut of five non-pheromone-producing cockroaches are

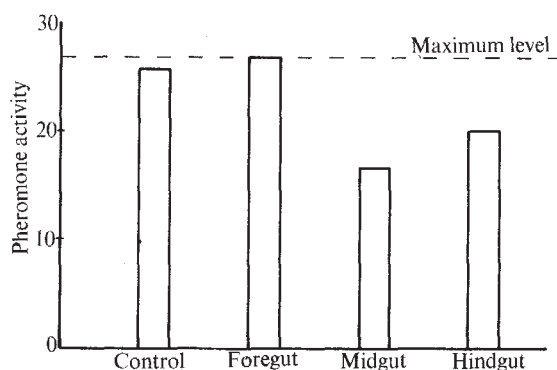


Fig. 1 Inactivating effect of gut from non producing females, 2 h.

significantly different from scores for mid and hind gut (at the 2.5% level).

Male gut preparations showed the same effect after a 2 h incubation with a marked increase in inactivation by the midgut at 24 h. Fig. 2 illustrates the same experiment using pheromone-producing females. The decrease at 24 h may be an artefact due to excessive aeration, which reduces pheromone activity, or to a decrease in responsiveness of the males which occasionally occurs.

The properties of the gut of pheromone-producing females were investigated further. Five portions of midguts dissected from fresh male cockroaches were added to 24 h preparations from pheromone-producing females. These male midguts were halved sagittally, one half added to the female preparations and the other placed in saline with pheromone. The combined preparations each showed greater inactivation than either of the corresponding single preparations, suggesting that the midgut of pheromone-producing females contains potential inactivating agent.

The appearance of the most inactivating agent in enzyme-containing tissues such as the mid and hindgut, and the fact that the inactivation effect increases with time, suggest an enzyme. Further experiments have shown that the factor is largely inactivated by exposure to 80° C for 15 min. Such considerations favour the involvement of an enzyme rather than the masking factor which Jacobson and Smalls found in crude extracts of whole *Periplaneta americana* in organic solvents⁸. For this reason an enzyme inhibitor, piperonyl butoxide (a gift of Niagara Chemical Division), which is known to act on the microsomal oxidase system of insect gut, was tested for its capacity to prevent pheromone inactivation (Fig. 3). In the assay used, the score for the males of each jar was the level of response rated on an expanded scale of 0 to 6 divided by the time in seconds to the first oriented response. In these conditions, the increase in pheromone activity in the midgut preparations after 3 days in the cold room is significant.

This result was sufficiently unexpected to suggest the further test of aerating and warming the preparations. After this treatment there was no difference between the controls and the midgut preparation. A possible interpretation is that in addition to inhibiting pheromone breakdown, piperonyl butoxide allowed the midgut to produce an unstable form of the sex pheromone (Fig. 3B).

Most insect pheromones seem to be products of externally ducted glands⁹, but some pass out in the faeces, arising, perhaps, from exocrine glands in the gut. These include the sex attractant of *Periplaneta americana*, the aggregation pheromone of *Blattella germanica*¹⁰, and pheromones regulating sexual development in the termite, *Kaloterms flavicollis*¹¹.

An alternative possibility is that pheromones produced by the gut are the products of normal digestive processes which have acquired significance during evolution, much as the specificity of hormones is said to be the result of the evolution of hormone receptors which became responsive to particular

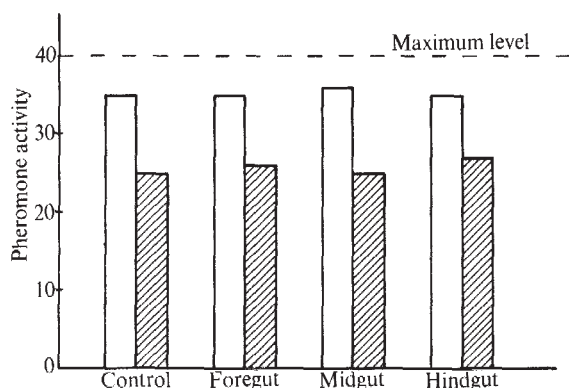


Fig. 2 Inactivating effect of gut from pheromone producing females: 2 h (white bars) and 24 h (hatched bars).

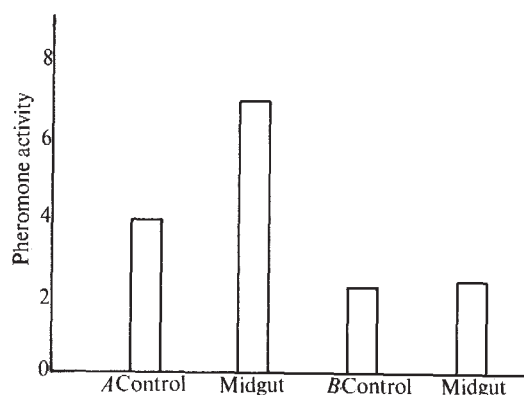


Fig. 3 Effect of 10^{-5} M piperonyl butoxide on pheromone inactivation by male midgut. Pheromone activity for each sample is the sum of ratios of level of activity over time in seconds to first oriented response for ten jars of cockroaches, that is $\sum \frac{\text{level of response}}{\text{time in seconds}}$. Each bar shows mean of ten samples.

A indicates assay after 3 days in cold room at 0°–4° C. B indicates assay after aeration and an additional 24 h at room temperature.

products of internal metabolism¹². The faeces could carry information, not only of an insect's metabolic and hormonal state, but also its recent feeding experience.

In such a system, gut enzymes could play a considerable role. The microsomal oxidase system of insect gut has been shown to be a versatile and complex system¹³. The above experiments suggest that this system is implicated in the regulation of at least one pheromone.

Wharton¹⁴ found that starved *Periplaneta americana* exposed to lethal doses of β -radiation had extensive gut damage. Virgin female cockroaches exposed to sub-lethal doses of this radiation showed an initial decline in sex pheromone production followed by a substantial overshoot⁵. Damage to the microsomal oxidase system of the gut is a possible explanation.

I thank Dr Louis Roth for interest and criticism. This research was accomplished during tenure of a National Research Council visiting scientist research associateship.

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Received June 2; revised July 24, 1972.

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BOOK REVIEWS

Man, Money and Mining

River of Tears: The Rise of the Rio Tinto Zinc Mining Corporation. By Richard West. Pp. 201. (Earth Island: London, 1972.) £1.95.

THIS book is not a work of science and a notice of it in *Nature* can only be justified for the interest it may have for some environmentalists. The title is based on an emotive phrase from a speech by President Kruger after the discovery of gold on the Witwatersrand: "Every ounce of gold taken from the bowels of our earth will have to be weighed against rivers of tears and the life blood of thousands of our comrades. . . ." It is a study of the international politics of mining today, as illustrated by the effect of Rio Tinto Zinc on countries in three of the five continents in which the company operates. The rise of RTZ is traced from early beginnings in the Huelva province of Spain, already desolated by centuries of pyrite and copper mining, to the amalgamation with the Zinc Corporation and the emergence of the company, under Sir Val Duncan, as what the author claims to be the world's largest mining concern. Whether this claim is right or not, the importance of RTZ as a British-based international enterprise is unquestionable. Its tendency to avoid the fierce glare of publicity is evidently deplored by Mr West, who has collected as much critical comment as he can from Africa, Australia, and the United Kingdom and gives the impression that he would genuinely have liked to have the opposing arguments. As it is, he endeavours to show that RTZ is bolstering up apartheid in South Africa by mining copper at Palabora, is supporting an illegal regime in South-West Africa by working uranium ore at Rossing and, by discovering nickel mines in Rhodesia, is ensuring the success of the Smith government. He further discerns a threat to the national sovereignty of the Commonwealth of Australia in the great RTZ bauxite operations at Weipa and the Japanese contracts for which RTZ (with other companies) have brought into production the huge iron ore workings of Hammersley Ranges. In Bougainville, he finds that the development of an open-pit copper

mine 3,000 feet long by 2,000 feet wide is destroying a demi-paradise. Finally, in Britain he seeks to show how the provision of new jobs by the erection of the lead-zinc smelter at Avonmouth and the aluminium smelter on Anglesey has been achieved at the expense of the environment, and he concludes with a review, commendably moderate in tone, of the conflicting views about the prospecting of the Mawddach Estuary and the mountainside at Coed-y-Brenin forest. There is much else besides, including a curious attempt to link RTZ, by way of the Concorde, with the old-time slave traders of Bristol.

It will be obvious by now that this is a book with a slant, but we are not given a clear statement of this until the end, when Mr West states his own position: "Wealth, or Mammon as it was once called, is worshipped still in the interests of 'growth' or greed, the metals are ripped from the earth to be used in motor cars, Concorde aeroplanes, beer cans, nuclear weapons and TV sets. Instead of conserving the world's dwindling natural resources, governments encourage the miners to still more frantic efforts. As a consequence of this mineral rush, farmers are thrown off their land, whole mountains are scooped away and their poisons dumped in the sea; industry invades the few last patches of natural beauty." If there is any constructive conclusion to this study, it is that we should seek to return to that blissful non-metallic state of early Neolithic time when the tool was made of flint or obsidian, the plough hacked from a dead tree, and untamed natural beauty was everywhere. How many, I wonder, of island earth's teeming millions could survive?

KINGSLEY DUNHAM

Cartesian Astronomy

The Vortex Theory of Planetary Motions. By E. J. Aiton. Pp. ix+282. (MacDonald: London; American Elsevier: New York, February 1972.) £6.

In 1644 the French philosopher René Descartes published the first full account of the system of natural law

that he had constructed in imagination during his earlier years. He conceived space to be everywhere occupied by matter. Hence, any motion of a body must be part of a closed system of displacements; and the Universe was to be conceived as an aggregation of revolving systems of particles, or vortices. The fiery centres of these vortices were the stars; the planets were defunct stars captured by adjacent vortices and thereafter carried round like straws in a whirlpool.

Historians of science commonly state that the vortex theory was eventually discredited by the gravitational synthesis of Newton, who, it is alleged, established that no vortex-borne planet could conform in its motion to Kepler's Laws. This, however, is to oversimplify a complex historical development; all the more welcome on that account is the authoritative re-assessment now presented by Dr Aiton.

After glancing at earlier, less precise vortical models, Dr Aiton traces the fortunes of the classic formulation by Descartes through what he distinguishes as its three historic phases. In the period ending with the publication of Newton's *Principia* in 1687, practical astronomers were content to base their work on Kepler's Laws, feeling no need of any facile physical theory to support them; while the Cartesians were equally satisfied with just such superficial explanations, taking no account of Kepler's Laws, with which, indeed, Descartes seems not to have been acquainted. The second phase, extending to about the time of Newton's death in 1727, was marked by Cartesian efforts to embrace the details of planetary motion in the vortical scheme while questioning the technical validity of Newton's criticisms of the Cartesian vortices. The third, post-Newtonian phase was marked by attempts to harmonize the essential principles of the two rival traditions; when this was found to be impracticable the Cartesian cause was lost.

Throughout the long debate the Cartesians enjoyed the advantage that they were referring the phenomena to known laws of impact, while the Newtonians appeared to be relying on

"attraction at a distance", an increasingly unacceptable "occult quality".

Dr Aiton has earned the gratitude of students for extracting and lucidly presenting the essence of his theme from a mass of obscure and frequently tedious source material.

A. ARMITAGE

Static View of Science

The Philosophies of Science: an Introductory Survey. By R. Harré. Pp. 191. (Oxford University: London and New York, January 1972.) £1.30 boards, 60p paper.

THE philosophy of science, as an academic discipline, is deeply divided among its practitioners into a number of schools, each of which accentuates its differences from and minimizes its similarities to the rest. The frequent confrontations between these schools have become a ritual. In spite of a general belief to the contrary, these confrontations are of minor significance in the progress of the field of study. A beginner—be he scientist, layman or student of philosophy—wishing to gain some insight into the philosophy of science will find very little literature setting out the basic positions and problems in the philosophy of science, and he is more likely to be irritated than instructed by much contemporary writing.

Mr Harré's book is subtitled as being an introductory survey to the philosophy of science. In many respects it is more successful than most surveys in giving a fairly balanced view of some of the major approaches to the subject. Having divided the subject into three branches, logic, epistemology and metaphysics, the author then delineates in turn three theories of the logical structure of science—inductivism, Popperian falsificationism and positivism—showing their respective strengths and weaknesses.

The second theme of this book encompasses the various epistemological paradigms used by scientists, natural philosophers and philosophers of science. Rather than characterizing science by a single epistemological model, the author shows that certain of these models are more appropriate to particular sciences than to others. Sciences such as chemistry and anatomy are presented as being more suited to a realistic interpretation while physics is offered as being more accurately described by the phenomenalist paradigm.

The role of metaphysical theories is the third theme discussed in this book. This is an important subject which has too frequently been neglected in introductory texts. Among the topics dealt

with are the discreteness or continuity of matter and time. Causation is discussed in terms of the generative and successive theories of causality. As this book deals with "classical" science the author emphasizes the former theory while omitting to mention the significance of the latter to modern physics. Aristotelian metaphysics is referred to briefly before the author considers the corpuscular world-view in greater detail, and the relation between the three themes—logic, epistemology and metaphysics—is illustrated by his analysis of the corpuscular philosophy.

In general, the author's style is lucid if dry in places. The introductory chapter is disjointed and lacks clarity and is further marred by a typographical confusion on page 17. An example of this lack of clarity is in the discussion of the status of scientific knowledge. The perplexed reader of this section is left in a state of limbo owing to the author's cavalier use of terms like knowledge, truth and certainty.

This book is certainly an admirable introductory text on logic, epistemology and metaphysics. But owing to this division of the subject, a grey and static view of science is presented, as if science consisted of nothing other than these three branches, and Kuhn had not written *The Structure of Scientific Revolutions*. G. N. CANTOR

Tragi-comedy of Science

The Call-Girls: a Tragi-comedy with Prologue and Epilogue. By Arthur Koestler. Pp. 191. (Hutchinson: London, October 1972.) £2.25.

DON'T worry: Arthur Koestler has not taken to porn—he breaks fresh ground. *The Call-Girls* is a novel, aptly introduced by his publishers with: "Off they go, the distinguished members of the academic call-girl circuit, to yet another cultural congress, scientific conference, inter-disciplinary symposium." Economy-class fares paid; a modest honorarium or maybe no honorarium. . . . Suppose I were to let fall the word Pugwash, for example—is there not a shock of recognition? I am afraid Mr Koestler has introduced an undying concept into our culture: I shall never again think of them as anything but the call-girls of Academe—summoned over the long-distance telephone by "some professional busybody, Foundation or university" to do their thing, that is assemble, each read a paper, dispute and discuss to a climax, ejaculate a resolution or message, and then disperse to await the next call.

Mr Koestler's brilliant symposium is sponsored by the International Academy of Science and Ethics and is held in a

Scandinavian-type glass-and-concrete Kongresshaus improbably and sadistically planted in a remote Alpine village: the title finally settled on is "Approaches to Survival"; but its aim, and Mr Koestler's aim, is no less than to discuss the ultimate viability of the human race, which—to me at least, and, I should have thought, to many people nowadays—seems open to doubt.

The date seems to be a few years from now. The symposium is led by Solovief, an American physicist who has been awarded a Nobel Prize for discovering a new fundamental particle: he limits the number of participants, which includes two more Nobel Laureates, to twelve. (There is a prologue in which Jesus of Nazareth, hauling the Cross up Calvary, calls on God, the Father, to show His hand—if any.) Each of the twelve represents a current, not to say in some cases modish, approach to man. They include a post-Skinnerite behaviourist; a post-Kleinian psychologist; a surrealistic, down-with-the-system nihilist; a post-Delgadoist brain-stimulator; a slick, self-advertising exponent of "controlled schizophrenia"; a blushing young priest to propound extrasensory perception and psychokinesis; a silly, old, distinguished poet, Laureate among call-girls and the sole representative of the arts, whose main contribution to the action is a pass at the blushing young priest. Each delivers a paper, in which Mr Koestler displays his mastery of the art of exegesis; and, because this is fiction, he can take advantage of not having to side with anybody other than, perhaps, Solovief.

So Mr Koestler has created a novel of intellectual disputation. And a good thing that is, too, in days when we seem only too frequently to be offered the alternative, in novels, between headless action and incoherent emanations of the author's inner world. I found myself reminded of the early Aldous Huxley. Mr Koestler does not possess Huxley's poetic gift, and *The Call-Girls* is less "written" in the Henry Jamesian sense: on the other hand he does not suffer from the intellectual weediness that strikes one if one re-reads say *Chrome Yellow* now.

The crunch, so far as criticism is concerned, is of course in the climax of the novel, the message; in which Mr Koestler, through having taken on the tallest of orders, has to deliver the tallest, and the most serious, of goods. Very, very difficult—I should have said impossible, myself. I shall not disclose Solovief's recipe for human survival—in order to make sure you read Mr Koestler's book to see what it is. What I will disclose is that, like everything Mr Koestler writes, it is fascinating and provoking.

WILLIAM COOPER

Anelasticity

Anelastic Relaxation in Crystalline Solids. By A. S. Nowick and B. S. Berry. Pp. xv+677. (Academic: New York and London, March 1972.) \$27.50.

THE names Nowick and Berry have long been intertwined with the evolution of both theory and experiment in anelasticity. This fine book—in preparation for half a dozen years—reflects the thorough knowledge and sensitive feeling they have for the subject.

The book contains two sections—the formal theory and examples of various anelastic phenomena. The theory builds on the old brief monograph by Zener, with whom Nowick worked as a post-doctoral fellow in the late 1940s. This theoretical treatment is excellent, bringing together for the first time many fragments of the subject published through the years. The short chapter on the thermodynamics of anelasticity is perhaps the only understandable treatment in existence—the brief intuitive discussion of the subject in Zener's monograph has not been very useful to most people and is rarely referred to.

The second section on anelastic phenomena is also well done. All important examples are mentioned and the state of thought for each is brought up to about 1970. These chapters are not merely a compilation of references to the topics; rather assessment is made of the state of understanding of each phenomenon. Experts might quibble about relative space given to the various sub-fields, but the balance seems about right.

One point of focus of this part of the book is establishment of a model for each relaxation. Thus, measurements of relaxation time and relaxation strengths are utilized more for elucidation of models for the relaxation than for their further use in metallurgy, ceramics or solid-state physics. For example, extensive use is made of measurements of the relaxation time and strength of the Snoek effect in defining the geometrical model of interstitials in bcc metals; much less space is devoted to the voluminous literature on use of the Snoek effect in deducing precipitation kinetics of carbides and nitrides in bcc metals. Space limitations were undoubtedly the reason for this and other exclusions.

Danger of error always exists in reviewing fields in which progress is rapid, a danger all the more acute in the publishing delays attendant on production of a hard-cover book. Examples of such error exist—the authors would certainly write the section on "Snoek peaks" due to hydrogen in metals quite differently now, for some of the interruptions they quote have turned out probably to be wrong.

Documentation the authors give to original papers is excellent: the excellent index of references will make the book useful as a catalogue of original works.

The book is well assembled. The figures are well done and the type style reads well. Altogether this will be a reference work of importance to research people and to graduate students; it should have a long, useful life.

CHARLES WERT

Plant Parasites

Biochemical and Ecological Aspects of Plant-Parasite Relations. Edited by Z. Kírály and L. Szalay-Marzó. Pp. 425. (Akadémiai Kiadó: Budapest V, Hungary, 1971.) £6.75.

THIS book contains the papers given at an international symposium held in Budapest in September 1970 to celebrate the 90th anniversary of the Hungarian Research Institute for Plant Protection. The papers are in four groups and deal with insect pests and diseases caused by viruses, bacteria and fungi. The first group of seventeen papers is on defence reactions of plants to infection and includes a wide range of topics, seven relating to viruses, six to fungal pathogens and four to pathogenic bacteria.

The second group of eleven papers, on the ecology of insect pests, includes a number on the effects of parasites on insect populations. There follow eleven papers on new approaches to pest control, mainly those that exploit the use of juvenile hormone analogues, "antifeedants" and chemosterilants.

The last section of thirteen papers is on the highly topical subject of systemic fungicides and how they act against plant pathogens. Most of the papers are about the use and mode of action of specific fungicides. There are also stimulating papers on more general aspects by two of the best known workers in this field.

The book has been attractively produced and is well illustrated. It is a little unfortunate that there is no index in view of the range of subjects covered and the very large amount of up to date information that is presented. This, however, is no more than a minor criticism of a most interesting and coherent collection of papers on four very important topics in crop protection that are currently attracting much attention. The editors are to be congratulated for their work in producing this volume especially because as Hungarians they edited forty-one papers in English and eleven in German to a standard that many whose first language was English would be glad to achieve.

R. K. S. WOOD

Chapel en le Frith

Geology of the Country Around Chapel en le Frith. By I. P. Stevenson and G. D. Gaunt. Palaeontology by M. Mitchell, W. H. C. Ramsbottom and M. A. Calver. Petrography by R. K. Harrison. Pp. xii+444+24 plates. (HMSO: London, December 1971.) £3.20.

THIS well produced and illustrated memoir is concerned wholly with Carboniferous rocks up to and including Lower Coal Measures and with the superficial unconsolidated Pleistocene and Recent deposits. The highly fossiliferous Carboniferous Limestone (Lower Carboniferous) and its contained basic igneous rocks made classic by continuing studies beginning with Martin, 1809, has a complex stratigraphy. There is a fair summary of the conflicting views which have been held and an account of the eventual proof of the correlation of the goniatite and "standard" corallachiopod zones of the upper part of the Lower Carboniferous as well as of the intricate relations of the shelf, marginal or reef, and basin facies. There are many well displayed sections in the deep cut gorges and these are fully described as well as exploratory boreholes made especially to elucidate the complexities since the interpretation has a considerable influence on the exploitation of the limestone and its contained minerals. The mineralization, confined to the limestone, provides important quantities of fluorite. The occurrence of the minerals is treated in summary since there is already an extensive literature.

The Millstone Grit Series up to 4,000 feet thick, which occupies a large part of the sheet area to the north of the Carboniferous Limestone outcrop, is given full treatment and, together with that on the adjoining sheet 100, can be regarded as the type development. This account will be the standard of reference for rocks of this age, Namurian, throughout the Pennine region and will serve as the comparative succession over a wide area. The Lower Coal Measures, confined to the Goyt Trough in the western part of the district, complete the "solid" succession.

The area is a fascinating one and a more extended discussion of its geological history might have been included although this will doubtless be forthcoming when it can be based on the completed mapping of the sheets to the south. The palaeontology is based almost wholly on macropalaeontology, which is admirably done, but the microfossils could be studied as a further aid to correlation using this sequence as a standard. Altogether a fine memoir providing a wealth of information for the quarrying, mining and water engineering interests as well as for academically based studies.

J. SHIRLEY

Talking to Machines

Understanding Natural Language. By Terry Winograd. Pp. vi+191. (Academic: New York and London; Edinburgh University: Edinburgh, May 1972.) \$8.95.

ASKED to say what artificial intelligence has to show for its first fifteen years, any practitioner is bound to say "Terry Winograd's program". Anyone who has seen the filmed dialogue between program and user — part of which is reproduced in this book — cannot fail to be impressed by the naturalness with which the computer program carries out commands, and provides informative answers both about the current state of its world, and recent events involving the program's manipulation of that world.

The world of the program is a large collection of formulae or "assertions" which can be operated upon by procedures. Collectively these procedures and assertions make up a data base which describes in a minimal fashion a table top with coloured blocks of various shapes and sizes on it, and a variety of actions and relations in that world. Each object is located on a three dimensional coordinate grid: thus (#AT:B3 (400 0 0)) asserts that a block whose "internal" name is "B3" is located at $x=400$, $y=0$, $z=0$. Assertions of the same general form are a prominent feature of the procedures which represent actions like GETRIDOF, PUTON, GRASP, MAKE-SPACE and so on, in such a way that a request to "put X on Y" can, after checking that (#ON X Y) is not already asserted (that is X is not already on Y), cause a search over the procedures to locate one whose asserted goal is (#PUTON X Y). Activation of this procedure creates new assertional requirements which may be satisfied in turn by other procedures and so on.

On this computational deep structure an account of natural language and its relation to intelligence and cognition is constructed which is at once more explicit, more detailed and fundamentally more plausible than anything else on the book shelves. It is more plausible because, at last, there is a honest attempt to grapple with meaning and reference regarded as a translation of the user's sentences into computational forms which have some chance of coping with behaviour in a real world. The requirements of this translation motivate a detailed linguistic theory which uses a "systemic" grammar to determine the form of the complex syntactic structures which are assigned in understanding the sentence. This departure from the more fashionable transformational grammar of Chomsky is strongly influenced by a concern to reflect the speaker/hearer's

knowledge of the language in the process of comprehension, not merely in the syntactic structures themselves. Winograd pursues this theme further by exploiting a version of the semantic theory of Katz and Fodor to smooth the path of efficient understanding. In his hands that theory is a heuristic technique which both reflects and merges into the speaker/hearer's knowledge of the world.

In the early 1960s Bar-Hillel "showed" the impossibility-in-principle of mechanical translation of natural languages by drawing attention to the way in which a knowledge of real world objects and relationships is involved in language use. He illustrated the point by drawing attention to the ambiguity of the word "pen" in the sentences "the pen is in the box" and "the box is in the pen". He concluded that such a disambiguation was impossible because one could not provide a machine with such knowledge of the real world or the capacities to use it. The requisite inferences here, having to do with relative sizes and locations, are precisely those captured by the computations performed in Winograd's blocks world: it is the pervasive influence of such varieties of inference in the understanding of utterances that is a central thesis of Winograd's work.

Having made much of the centrality of meaning and reference it may disappoint the reader to find that a third of the account of the system is devoted to a grammar of English and even more surprising that the fundamental driving force is provided by the parsing of the utterance. The perceptual and manipulative actions in the blocks world, generated by the understanding of the user's utterance, are in a strict computational sense merely a side effect of a successful parse: an ideologically unsound but pragmatically successful tour de force. Of deeper concern to those who believe that utterances are much messier than the linguist appears to believe, is that despite overt deployment of meaning and reference it is still necessary that utterances be grammatically well formed and words correctly spelled.

These points should not be thought of as "defects" so much as problems whose solution will require a basic restructuring of the processes captured in the program. Winograd's achievement is to show how knowledge of the language and of the world can (and must) be expressed in the process of understanding just as much as in the structures (syntactic and semantic) upon which that process operates. It is this which distinguishes Winograd's work from the contemporary linguistic orthodoxy ushered in by Chomsky's *Syntactic Structures* and makes it required reading for any self respecting

student of human nature.

The texts of the two editions of this book are identical save for the addition in the Edinburgh University Press copy of an index and a brief preface by the author.

MAX CLOWES

Enumeration

Development of Modern Mathematics. By J. M. Dubbey. Pp. 7+145. (Butterworth: London, November, 1970.) 50s boards; 28s paper.

IN recent years the history of mathematics has come to assume an importance much greater than at any time in the past. It is now a fashionable subject, both in its sociological and cultural aspects, and is becoming of increasing interest as a possible component in the curriculum at schools and colleges. But it is more than this. It is important in mathematics as such, both because of the tremendous developments of the past two centuries or more and also because of the great emphasis in contemporary mathematics on concepts and their relationships. Clearly in such an emphasis there is a rightful place for a study of the historical development of the concepts.

In relation to these developments the appearance of the book under review is most timely. There is, in fact, a great dearth of literature on the history of mathematics, apart from a number of classical texts on Greek mathematics, and a noteworthy feature of this book is that more than two-thirds of it is given to mathematics as it has developed since the time of Newton. Particularly important are the chapters on eighteenth century mathematics and on the origins of modern mathematics. The author bravely attempts an introductory survey of twentieth century mathematics and it is understandable that he "trembles at the prospects". Under pure mathematics he has written a good, but brief, survey of ideas in analysis and in analytical topology, but a notable absence is any reference to the widespread developments in abstract algebra during the present century. As for applied mathematics there are brief references to computers and techniques, but a noticeable omission of any reference to some of the main branches of the subject, for example, fluid mechanics and electromagnetism. Having said this, it is only fair to say also that the book will give the reader a very good first impression of some of the main mathematical developments over recent decades. It should prove especially useful on undergraduate courses in the history of mathematics, provided the students already have a reasonable background in the mathematics whose history they are studying.

L. S. GODDARD

CORRESPONDENCE

British Astronomy

SIR,—In his comments on Dr Burbidge's account of British optical astronomy, Dr Fellgett writes that the large reflectors of the American West Coast were developed from Mr Common's 36-inch reflector which was sent from Halifax to California in 1890. In my view it would be currently more useful for an optical astronomer to use Mr Common's reflector than to use all of Dr Fellgett's novel instrumentation¹ with the 98-inch Isaac Newton telescope in Sussex. Climate comes first.

Despite this, a first class observer confined to Sussex would be more likely to make a significant discovery than an inferior observer in California. Selection and training of good observers come only second to climate, and the UK can be pleased with the results of its PhD training programme. This may be because it is elitist and geared to the best students.

Most of these PhDs have understood the truth of the first paragraph, and they are now to be found in the US or South Africa. But travel is an insignificant part of the total cost of running a major observatory, and travel time is not large compared with total time at the observatory. So there is no reason why these astronomers should not operate out of the UK, using telescopes in a favoured site.

However, research funds are very limited in the UK partly because it tries to support too many observers. So the better of these observers have found overseas positions with good facilities, adequate funds for all worthwhile programmes, and a standard of living double or triple that offered them in the UK.

There are two practical alternative solutions. The UK can retain the top group of its new astronomers, and let the rest of the world have the remainder. To do this it would have to create a front rank observatory, competitively funded for facilities, new programmes, and astronomers' standard of living. Or it could get out of optical astronomy and close its PhD training programmes. It certainly cannot afford to continue to train students while other countries reap the benefit. It is characteristic of the Peter Pan technique of UK science planning that the reality of this decision has not been faced. This is the meaning of the battle for creation of a Northern Hemisphere Observatory.

Yours faithfully,

NEVILLE J. WOOLF

School of Physics and Astronomy,
148 Physics Building,
Minneapolis, Minnesota 55455

¹ *Nature*, **239**, 239 (1972).

Apologies to *Xenopus*

SIR,—Owing to an unfortunate delay in my obtaining past issues of *Nature*, I only recently had the opportunity of seeing them. I was surprised to read (*Nature*, **237**, 198; 1972), "... in *Xenopus laevis* (the notorious midwife toad)." On checking your subsequent numbers it seems that you have not printed a correction, together with your profound contrition, for your correspondent's ghastly mistake. As *Xenopus* is well known for its use in pregnancy testing, the error of mistaken identity can well be understood, even by the midwives, but in order to placate Messrs Koestler, Nieuwkoop and Faber and Miss Elizabeth Deuchar, who have extensively reviewed the animal called the South African clawed toad, and all developmental biologists, for whom *Xenopus laevis* is almost a patron saint, I beg you to issue a formal correction. Please allow the tragic Paul Kammerer to rest easily in his grave and state that the notorious midwife toad is *Alytes obstetricans* not *Xenopus laevis*.

Your neurophysiology correspondent should go on bread and water for at least 24 hours.

Yours faithfully,

HAROLD FOX

Department of Zoology,
University College London,
Gower Street,
London WC1E 6BT

¹ Deuchar, E., *Biol. Rev.*, **47**, 37 (1972).

² Koestler, A., *The Case of the Midwife Toad* (Hutchinson, London, 1971).

³ Nieuwkoop, P. d., and Faber, J., *Normal Table of *Xenopus laevis** (North Holland, Amsterdam, 1956).

Proper Use of Errata

SIR,—The necessity for errata to scientific publications is increasing with the increase in publications which do not send proofs to authors. The problem is how to make readers aware of errata without special bibliographical exploration.

As a librarian in two scientific laboratories, I tried the following experiment: noting all the errata in the periodicals arriving at one of my libraries (about sixty subscriptions), I glued a reminder on the margin of each of the original papers signalling the existence of errata and where to find them. It took me about 20 minutes a week to note all the errata in the periodicals arrived during that week and a little more to glue the reminders.

I completed the work for all the issues not yet gone to the binders and began to explore the past. In a fortnight, all the

current year was done as well as five years of the *Journal of the American Chemical Society* and four years of the *Journal of Chemical Physics*. In one further week, all the errata of the *Journal of Chemical Physics* since volume 32 (1960) were noted, organized in chronological order of appearance of the original papers and photocopied, ready to be inserted in the relevant volumes.

I am now trying to organize the exchange of such photocopies for all periodicals arriving in the science libraries on the Orsay campus. I propose that this suggestion be followed by many scientific librarians and that the sheets of errata be at the disposition of other librarians so as to avoid the duplication of work. I would be very grateful if librarians would write to me about this suggestion and propose to take care of the noting of errata of this or that periodical.

The final objective is that the scientific periodicals publish themselves similar slips ready to be photocopied by librarians who would then have only to cut them out and glue them on the margin of the relevant papers.

Yours faithfully,

J. HADAMARD

Laboratoire de Photophysique Moléculaire,
Laboratoire de Physicochimie des
Rayonnements, Bâtiment 350, 91-Orsay

Thymineless Death

SIR,—In his review of the history of the stringent and relaxed response (*Nature*, **238**, 370; 1972) your correspondent writes "the characterization of this control system proceeded slowly and sometimes painfully until 1961". Your readers may be interested in reading an accurate historical footnote why progress was slow and painful. I reported at a Federation meeting in 1954 the discovery of what came to be known as the relaxed control. My paper followed that of my distinguished current colleague, Seymour S. Cohen, on thymineless death. We had a total audience of four, his two technicians and my two technicians. Publication of the full paper was delayed because it was rejected by the leading journal of biochemistry and biophysics as being an unimportant observation. It was finally published in *Journal of Bacteriology*, **69**, 460 (1955).

Yours faithfully,

ERNEST BOREK

Department of Microbiology,
University of Colorado Medical Center

Final Decision

SIR,—When I read Professor McCrea's suggestion (*Nature*, **239**, 239; 1972) for limiting scientific publication by a scheme to be called ACRE, I wondered whether he had intended it in satiric vein. Dr O'Connor's suggestion (*Nature*, **239**, 418; 1972) of calling for support by postcard, before full publication is made, raises a number of serious objections.

Authors in big departments or with a wide circle of friends would doubtless tend to receive more support than others. It would set an extra hurdle in the path of the unorthodox. Authors in fields with few workers might not receive the necessary support in the popularity poll. It would surely accentuate the offering of inferior material to journals with less critical editors.

Over recent years, there has been a tendency, albeit with exceptions, towards shorter papers but it depends greatly on the topic whether argument should be in full and data briefly summarized, or the reverse.

Let us not put ourselves in an unnecessary straitjacket but leave it to editors to assess quality rather than quantity or popularity.

Yours faithfully,

E. A. GEORGE

*Culture Centre of Algae and Protozoa,
38 Storey's Way,
Cambridge CB3 0DT*

DDT and Peregrines

SIR,—It would have been more enlightening if D. L. Gunn (*Nature*, **239**, 296; 1972) had specified why the evidence I quoted (*Nature*, **238**, 475; 1972), linking the decline of peregrine with the persistent pesticides, was unsound, or had submitted the evidence to the contrary. Instead he says, without quoting the specific references, that "the evidence was examined by the Wilson Committee¹ and the Mrak Commission² and found to be inadequate".

I commented earlier on his selective use of the Wilson Committee findings and he gives no reasons now for regarding as "not altogether convincing" their suggestion that dieldrin was responsible. The Mrak Commission dealt with the decline of peregrine and other species at some length and concluded (p. 212), "These experiments appear to forge the last link in the chain of evidence that DDT and its derivatives have been the direct and principal cause of widespread and significant reductions in bird populations".

Yours faithfully,

STANLEY CRAMP

*32 Queen Court,
London WC1N 3BB*

¹ *Advisory Committee on Pesticides and other Toxic Chemicals*, 148 (HMSO, 1969).

² *Report of the Secretary's Commission on Pesticides and their Relationship to Environmental Health*, 677 (US Department of Health, Education and Welfare, 1969).

Obituary

Professor Trevor Ian Shaw



TREVOR SHAW was born in 1928 and educated at Bootham School, York, and at Clare College, Cambridge, where he read the Natural Sciences Tripos and where he also studied parts one and two of the Bachelor of Medicine degree. After graduation in 1950 he remained at Cambridge on an MRC studentship to undertake research at the Physiological Laboratory under the supervision of Sir Alan Hodgkin, PRS. On completing the work for his doctorate in 1953 he was called up for National Service and seconded to the Medical Division of the Chemical Defence Experimental Estab-

lishment at Porton until his release from the army in 1955. For the next 11 years he worked at the Plymouth Laboratory of the Marine Biological Association of the United Kingdom, at first on a grant from the Atomic Energy Authority to study the uptake of fission products by marine organisms and then, from 1958 onwards, as a physiologist on the MBA staff. These years at Plymouth were both happy and productive and were marked by a succession of original discoveries which reflected his ability to deal with a number of fundamental problems in biology. In 1966 he was appointed Professor of Zoology in London University and spent the next six years at Queen Mary College. His warm and unassuming character, immense knowledge and infectious enthusiasm won him an ever widening group of appreciative undergraduates and colleagues. He was fully involved in his work up until his premature death on September 26, 1971.

Research work was a central part of Trevor Shaw's life and he was among the first to demonstrate, on red blood cells, a coupling between the transport of sodium and potassium ions. Later he elucidated the mechanism by which seaweeds accumulate iodine but above all he will be remembered for his sustained researches on the properties of nerve membranes. He was a brilliant experimentalist and devised, with Dr P. F. Baker, a remarkable method of

perfusing the giant axon of the squid with artificial media. This allowed the complete control of the contents of an active nerve fibre, a facility which Shaw and Baker together with Sir Alan Hodgkin and other collaborators have brilliantly exploited during recent years.

This distinguished work on ion movements and energetics was only one of Trevor Shaw's many research activities. He was the first to show, with Dr F. Crescitelli, that the photosensitive visual pigments are circularly dichroic and that this dichroism disappears on bleaching the pigments. He also did very original work on the buoyancy of squid and gelatinous animals such as jellyfish and ctenophores. In the early months of this year he embarked on a new and promising line of research in which he exploited the peculiar properties of laser light to allow the detection of movements of subcellular particles in living nerve cells.

Throughout his research career Trevor Shaw collaborated with some of the most famous physiologists in the country. It was easy to see how these collaborations developed, for he had an insatiable interest in research and a tremendously cooperative approach throughout his whole life. He was a gifted theoretician and never happier than when calculating the exact implications of a new idea. It was typical of his intellectual energy that he should spend hours with an insurance agent.

who called to sell him a policy, revising the man's method of calculating premiums and his gentle humour was such that he would later realize the comedy of the situation and delight in relating such tales. The unselfish help which he gave to his colleagues made him a most popular and valuable member of the scientific community. He served on the Councils of the Society for Experimental Biology, the Marine Biological Association and the Freshwater Biological Association and was a committee member of the Physiological Society, the British National Committee for Biology and the British National Committee for Biophysics.

Trevor Shaw's death will be a serious loss to the University of London. Within the period of his tenure of the Chair of Zoology at Queen Mary College he had built up a strong group of experimentalists among his associates and had working ties with engineers, physicists, mathematicians and chemists. He was a scientific polyglot able to converse freely with a wide range of colleagues and he made a unique contribution to the development and teaching of both undergraduate and postgraduate science at the college.

Throughout his life he was in great demand as a lecturer. He visited Leiden, Hamburg, Rome, Illinois, Alabama, the

Max Planck Institute, the College of Physicians and Surgeons at Columbia University and spent extended study periods at UCLA and Duke University, USA, and at Montemar in Chile. Early this year he represented the Royal Society in discussions in Japan with the Japan Academy. He was a Scientific Medallist of the Zoological Society and was elected FRS in 1971.

The distinction and dedication shown in Trevor Shaw's contribution to science will be missed by many. Those who were also fortunate to know him will greatly miss a dear friend who had the warm and generous nature of a most unselfish man.

Errata

THE following corrections should be made to the article "Selective Feeding of *Anopheles gambiae* according to ABO Blood Group Status" by Corinne Shear Wood, G. A. Harrison, Caroline Doré and J. S. Weiner (*Nature*, **239**, 165; 1972). The equation at the top of column 2, page 165, should read:

$$s.n.d._i = \frac{\log_e [(r_0 + \frac{1}{2}) / (r_A + \frac{1}{2})]}{[(r_A + \frac{1}{2})^{-1} + (r_0 + \frac{1}{2})^{-1}]^{\frac{1}{2}}}$$

The sentence which starts on line 12 beneath this equation should read "On the null hypothesis that mosquitoes feed randomly with respect to ABO status, the s.n.d.s have a mean of 0 and the standard deviation of the mean equals $1/n^{\frac{1}{2}}$."

In the News and Views note "Successful AID" (*Nature*, **238**, 430; 1972) the period of gestation of artificially inseminated reindeer was incorrectly given as 126 days; the correct figure is 216 days.

In the article "Oceanic Fine Structure" by H. Charnock (*Nature*, **239**, 32; 1972), the illustration labelled Fig. 1a should be Fig. 1b and vice versa.

THE following corrections should be made to the article "How Bees Navigate" by Allan W. Snyder and Colin Pask (*Nature*, **239**, 48; 1972): the footnote reference numbers at the end of paragraph 1 should be replaced by reference 6; footnote reference 7 should be inserted at the end of paragraph 4 and at the end of the legend to Fig. 3; footnote reference 7 in the legend to Fig. 5 should be replaced by reference 9; the first full paragraph on page 50 should read: "It is relevant that bees respond most strongly to blue-ultraviolet electromagnetic waves^{1,15,16}, which are not only the most important frequencies in polarized sky light¹⁰, but are also those which produce the most pronounced electromagnetic mode rejection effect. The structure of the drone bee eye¹³ is different from that of the worker.

Furthermore, the drone also has a low resistance between reticular cells¹⁴ (unlike the worker). These differences may reduce the mode detection. Because the drone rarely leaves the hive, there is no reason for it to have evolved special navigational aids."; footnote reference 16 at the end of the penultimate paragraph should be replaced by 17; reference 6 should read Snyder, A. W., and Pask, C., *J. Comp. Physiol.*, **78**, 346 (1972); reference 7 should read Snyder, A. W., and Pask, C., *J. Opt. Soc. Amer.*, **12**, 1389 (1972); reference 16 should be changed to reference 17, and a new reference 16 inserted as follows: Gribakin, F. G., *Vision Res.*, **12**, 1225 (1972).

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